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Major P. A. MacMAHON, D.Sc., Sc.D., LL.D., F.R.S., PRESIDENT,
in the Chair.

Frederick Chamberlin, LL.B. (Harvard), F.R.Hist.Soc.,
F.R.G.S., M.R.I., 6 Beulah Hill, Upper Norwood, S.E. 19.
Shankar Rao Ullal Savoor, Trinity College, Cambridge; and
Dr. Evert Jan baron de Vos van Steenwijk, The Observatory,
Leiden, Holland,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows
of the Society, the names of the proposers from personal knowledge
being appended :—

Godfrey Harold Hardy, M.A., F.R.S., Trinity College, Cam-
bridge (proposed by A. S. Eddington);

George William Jones, M.A., B.Sc., LL.B., F.R.S.E., Lieut.
R.N.V.R., 4 Bassett Road, London, W. 10 (proposed by
E. W. Barlow);

Francis Everard Lamplough, M.A., c/o Chance, Bros. &
Co., Lighthouse Works, Birmingham (proposed by A. S.
Eddington);

John Logie, M.A., B.Sc., F.R.Met.Soc., Sub-Lieut. R.N.V.R.,
1 Eversley Terrace, Green Lane, Goodmayes, Essex (pro-
posed by E. W. Barlow); and

Mear Rabson, M.A., Senior Mathematical Master, Whitgift
School, Croydon, 7 Burlington Avenue, Kew Gardens,
Surrey (proposed by Selig Brodetsky).

The following were proposed by the Council as Associates of the Society :—

Frederick Hanley Seares, Solar Observatory, Mount Wilson, California, U.S.A. ;

Harlow Shapley, Solar Observatory, Mount Wilson, California, U.S.A. ; and

Joel Stebbins, Professor of Astronomy, University of Illinois, Urbana, U.S.A.

Fifty presents were announced as having been received since the last meeting, and the thanks of the Society were returned to the respective donors.

The Secular Acceleration of the Sun as determined from Hipparchus' Equinox Observations; with a Note on Ptolemy's False Equinox.
By J. K. Fotheringham, M.A., D.Litt.

It is well known that the ancient eclipses of the Sun and the magnitudes of the ancient eclipses of the Moon yield a positive correction to the longitude of the Sun measured from the Moon's node as determined from modern observations and theory. While most investigators have corrected the motion of the Moon's node to account for these phenomena, Dr. Cowell has since 1905 * preferred to assume an acceleration of the Sun's motion, and Newcomb has taken the extreme course of setting aside the evidence as too untrustworthy to furnish corrections to the motion either of the Sun or of the node, though he has regarded the recorded times of the ancient lunar eclipses as good evidence on which to base corrections to the Moon's longitude. In 1915 † Miss Longbottom and I determined the secular acceleration of the Moon's motion in longitude from the ancient occultations ; we found a value substantially larger than the acceleration resulting from the times of the ancient lunar eclipses and inferred that the only possible explanation of the discrepancy was a secular acceleration of the Sun's motion which affected the times of the eclipses, but which had no influence on the times of the occultations. On comparing the solar acceleration thus obtained with the acceleration as determined from the magnitudes of lunar eclipses, we found a small correction to the acceleration of the node, but it was accompanied by so large a probable error that we expressed a doubt whether any correction to the theoretical acceleration of the node was necessary.

In the present paper I attempt to determine the Sun's acceleration independently of possible corrections to the lunar theory.

* *Monthly Notices*, 66, 3-5.

† *Ibid.*, 75, 377-396.

It may be well to avoid possible misunderstandings by explaining at the outset of this inquiry that by the phrase "acceleration of the Sun" I refer merely to the acceleration of the Earth's motion of revolution as compared with its motion of rotation, which we adopt as the standard of time, and do not wish to commit myself to any view as to the extent to which the phenomenon under discussion is due to a physical acceleration of the one motion or a physical retardation of the other motion. I also wish it to be understood that I am dealing only with the mean acceleration between the time of Hipparchus' observations and the time of the observations by which the present mean motion of the Sun is determined. I do not express any opinion on the question whether this acceleration has been uniform or subject to fluctuations. For the purpose of this paper it will be assumed that Newcomb's mean longitude and mean motion of the Sun are correct for the epoch +1800 Jan. 0.0. Any inexactness in this assumption can hardly affect the acceleration resulting from this discussion by more than a tenth of a second of arc per century, a small quantity compared with the probable error attaching to the quantity which I seek to determine.

Hipparchus' Equinox Observations.

In the first chapter of the third book of the *μαθηματικὴ σύνταξις** or *Almagest*, Ptolemy cites from Hipparchus' treatise on the Displacement of the Solstitial and Equinoctial Signs a series of twenty equinox observations, presumably made by himself, which Hipparchus had used in the discussion of the question whether the length of the year is uniform or whether it is subject to an inequality such as would be produced by an oscillation of the equinoctial points. I insert a translation of the passage.

"Then he sets forth first the times of autumn equinoxes observed with the greatest possible accuracy, (1) in the 17th year of the third Callippic cycle on the 30th of Mesore about sunset. (2) Three years later in the 20th year on the first of the supplementary days in early morning, though it should have been at midday, so that there was a discordance of a fourth part of a day. (3) A year later in the 21st year at the sixth hour, which was in agreement with the preceding observation. (4) And after eleven years in the 32nd year between the third and the fourth supplementary days at midnight, though it should have been in early morning, so that again there was the discordance of a fourth. (5) And after one year in the 32nd year on the fourth of the supplementary days in early morning, which was in agreement with the preceding observation. (6) And after three years in the 36th year on the fourth of the supplementary days at evening, though it should have been at midnight, so that again there was a discordance of the fourth [of a day] only.

* Ed. Heiberg, i. (1898) pp. 195, 196.

"And after this he sets forth also the spring equinoxes observed with similar accuracy: (7) in the 32nd year of the third Callippic cycle, on the 27th of Mechir in early morning: the *κρίκος* at Alexandria, however, he says, was equally illuminated on both sides about the fifth hour; so that here two observations of the same equinox differed by five hours approximately. (8) to (12) And the following equinoxes, he says, down to the 37th year are in accordance with the excess of one-fourth [*i.e.* with a value of the solar year exceeding the Egyptian year of 365 days by one-fourth of a day]. (13) to (19) And after eleven years in the 43rd year on the 29th of Mechir after the midnight at the beginning of the 30th the spring equinox took place, which was also in agreement with the observation in the 32nd year and accords also, says he, with the observations in the following years down to the 50th year; (20) for [in that year] it took place on the 1st of Phamenoth about sunset one day and three-fourths approximately after the equinox in the 43rd year, an interval which is divided over the seven intervening years."

References to the Callippic cycle are too numerous to leave any doubt as to the interpretation of these dates,* and it is remarkable that the whole series of observations should frequently have been placed one year too late. A careful reduction of these, as of all other dates in the *μαθηματικὴ σύνταξις*, will be found in Manitius's translation.† Manitius reckons his days from midnight and adopts the historical method of reckoning years before Christ. I have substituted astronomical days reckoned from Greenwich mean noon and negative numerals for the years, so that - 161 is equivalent to 162 B.C.

It will be observed that the whole list of observations is impersonal. That is probably only a matter of grammatical style. The fourth and seventh of the series are stated later in the same chapter‡ to be specially indicated by Hipparchus as having been most reliably observed by him, though elsewhere the reference is once more impersonal.§ There seems, therefore, to be no reason for questioning, as is sometimes done, the right of Hipparchus to be regarded as the observer of the first three equinoxes in the series. Of which of his predecessors would so severe a critic as Hipparchus have said that his observations were made with the greatest possible accuracy? An opposite tendency may be traced in those who infer from Hipparchus' citation of the observation made with the *κρίκος* at Alexandria that he must have been on a visit to Alexandria at that time. His express statement that the rival observation was "most reliably observed by him," and his

* Ptolemy actually gives the interval in years and days between two of these observations and two dated observations of his own; *ubi supra*, pp. 204, 205.

† 1912, 1913, 2 vols. It is curious that Manitius himself should have given the false dates in his edition of Hipparchus' *Commentary on Aratus* (1894, p. 282).

‡ Ed. Heiberg, p. 203.

§ *Ibid.*, p. 204.

use of it in deducing an oscillation of the equinox, are clear evidence that the observation on the *κρίκος* was derived from some source which he regarded as less trustworthy. There is one instance preserved to us where Hipparchus cites the magnitudes of a solar eclipse as observed both at the Hellespont and at Alexandria; he cannot have observed the eclipse at both places, and it is most probable that he observed it at neither.* The astronomers of his time must have been in the habit of communicating their observations when occasion demanded.†

It will be observed that Hipparchus is content with recording the observed times of the equinoxes to the nearest quarter of a day, and it is noteworthy that Ptolemy in commenting on the observations regards an error of this magnitude as possible. It is not merely possible, but certain that some of the observations are affected by errors of this magnitude; but in view of modern methods of dealing with errors, we may perhaps regret the scientific caution which deterred Hipparchus from recording the time more exactly. Ptolemy, though he knew himself to be an inferior observer, does not shrink from giving the results of similar observations of his own to the nearest hour.

In the passage cited above no mention is made of the method by which the observations were made apart from that made with the *κρίκος* at Alexandria, and I observe that Newcomb‡ seems to have supposed that Hipparchus determined the times of the equinoxes by observing the dates when sunset was exactly opposite to sunrise. As some of the equinoxes are stated to have happened at midnight, it is clear that the results must have been obtained by interpolation, and Ptolemy,§ who had the whole discussion before him, refers to the error in the observed declination of the Sun which would arise from a hypothetical error of 6' on the declination circle due to an error in the setting or graduation of the instrument. This leaves no doubt that the time of each equinox was obtained by interpolation between two observations, presumably meridian observations, of the Sun's declination.|| I regret that in an earlier paper¶ I endorsed an erroneous opinion that these observations were vitiated by refraction.

Postponing consideration of the observation made with the

* See Hultsch, *Hipparchos über die Grösse und Entfernung der Sonne, Berichte der sächsischen Gesellschaft*, 52 (1900), *Phil.-hist. Classe*, pp. 169-200, or my paper in *Monthly Notices*, 69, 204.

† The views combated in this and the previous paragraph have been copied from book to book. I have thought it unnecessary to trace them to their source.

‡ *Compendium of Spherical Astronomy* (1906), p. 253. His words do not expressly assert this view, but they at least suggest it.

§ *Ubi supra*, p. 197.

|| Ptolemy's own equinox observations were made with an instrument which he had specially devised for meridian observations; *ubi supra*, p. 203, and compare pp. 64-67. We may assume that Hipparchus was equally alive to the advantage of making observations on the meridian. The point is not absolutely essential to my discussion.

¶ *Monthly Notices*, 75, 378.

κρίκος at Alexandria, and confining myself to the twenty observations of the Sun's declination, I form the following table, where the first column gives the reference number of each observation, the second the observed date of each equinox reduced to Greenwich mean time, the third the apparent longitude of the Sun at that date, computed from Newcomb's *Tables of the Sun*, and the fourth the correction to Newcomb's apparent longitude resulting from the observation. In reducing the observed times I have assumed that all the observations were made at Rhodes (east longitude $28^{\circ} 14'$; the difference between Rhodes and Alexandria is unimportant for the present purpose) and that the observed times are local solar time. Ptolemy only applies the equation of time when dealing with observations of the Moon; there is no reason to suppose that Hipparchus would apply it when he did not attempt to define the time more exactly than to the nearest quarter of a day. I have not, however, introduced an artificial difference into the observed times by computing the small variations in the equation of time at different autumn or different spring equinoxes. The Sun's true longitude was computed by the method of which an example is given on p. 32 of Newcomb's *Tables of the Sun*, in which planetary terms and nutation are ignored. The apparent longitude was found by applying a constant of $-21''$ at each autumn equinox and of $-20''$ at each spring equinox.

TABLE I.

No.	Date.	Tabular Longitude.			Observed - Tabular Longitude.
		d	h	m	
1	-161 Sept. 27	4	0	180 30 52	-30 52
2	-158 Sept. 26	16	0	180 17 14	-17 14
3	-157 Sept. 26	22	0	180 17 41	-17 41
4	-146 Sept. 26	10	0	180 7 35	-7 35
5	-145 Sept. 26	16	0	180 8 2	-8 2
6	-142 Sept. 26	4	0	179 54 21	+5 39
7	-145 Mar. 23	16	15	359 30 55	+29 5
8	-144 Mar. 22	22	15	359 31 23	+28 37
9	-143 Mar. 23	4	15	359 31 50	+28 10
10	-142 Mar. 23	10	15	359 32 17	+27 43
11	-141 Mar. 23	16	15	359 32 46	+27 14
12	-140 Mar. 22	22	15	359 33 14	+26 46
13	-134 Mar. 23	10	15	359 35 58	+24 2
14	-133 Mar. 23	16	15	359 36 26	+23 34
15	-132 Mar. 22	22	15	359 36 53	+23 7
16	-131 Mar. 23	4	15	359 37 21	+22 39
17	-130 Mar. 23	10	15	359 37 49	+22 11
18	-129 Mar. 23	16	15	359 38 16	+21 44
19	-128 Mar. 22	22	15	359 38 44	+21 16
20	-127 Mar. 23	4	15	359 39 11	+20 49

The only error in the modern tables sufficiently large to be detected by an analysis of these observations is in the secular acceleration of the Sun's mean motion. The ancient observations must be regarded as subject to a constant error in declination which would include the effect of any constant error in the setting or graduation of the instruments and the effect of the neglect of refraction. We shall have to consider later the effect of expressing the result to the nearest quarter of a day only; this indefiniteness of expression will affect the probable error of the result, but does not provide any additional term which can be determined by an analysis of the observations. There appears to be no other error which would have a systematic effect on the result. I have, therefore, analysed the observations for two unknown quantities, (1) the secular acceleration of the Sun, (2) the constant correction to be applied to Hipparchus' declinations.

If s be the secular acceleration expressed in seconds of arc, ϵ the obliquity of the ecliptic, g the Sun's mean anomaly, T the time measured in Julian centuries from 1800 Jan. 0.0, and $\odot$ the longitude of the Sun, the correction to the Sun's declination due to secular acceleration will be $sT^2 \sin \epsilon \cos \odot (1 + 0.0351 \cos g + 0.0004 \cos 2g)$, where $\cos \odot = -1$ at autumn equinoxes and $+1$ at spring equinoxes. I have computed the obliquity of the ecliptic from the formula

$$\epsilon = 23^\circ 27' 31''.85 - 47''.090T - 0''.009T^2 + 0''.0018T^3,$$

where T is the time measured in solar centuries from 1850.0. In this formula the obliquity at epoch and centennial motion are taken from Dr. Brown's paper in *Monthly Notices*, 75, 507, 508 (1915). The same value for the centennial motion may be obtained by multiplying Boss's value for planetary precession in longitude as given in his *Preliminary General Catalogue* (1910) by $\cot N \tan \epsilon$, where N is the angular distance of the instantaneous axis of rotation of the ecliptic from the equinox. The coefficients of T^2 and T^3 were obtained by computing the centennial motion with Dr. Brown's correction to obliquity and Boss's corrections to precession for successive epochs separated by half-centuries from 1750 to 2100, and performing a least squares analysis for two unknown quantities varying respectively with the square and cube of the time. The formula was obtained with a view to all work on ancient observations; a less exact value would have been sufficient for the present purpose.

The value of g was taken from the quantity M in Newcomb's *Tables of the Sun* by the formula, $g = 0.9856003 (M - 5370)$. The differences between observed and tabular longitude were converted into declination by the factor $\sin \epsilon \cos \odot$. All work except the computation of g was done with four-figure tables. That is the reason why the last figures in the next table do not range. Putting δ for the constant error in Hipparchus' declinations, I thus obtained the following equations of condition:—

TABLE II.

1	- 157.08	- δ	= + 745.0
2	- 156.48	- δ	= + 415.9
3	- 156.38	- δ	= + 426.8
4	- 154.58	- δ	= + 183.0
5	- 154.48	- δ	= + 193.8
6	- 153.88	- δ	= - 136.4
7	+ 150.28	- δ	= + 701.8
8	+ 150.08	- δ	= + 690.5
9	+ 149.88	- δ	= + 679.8
10	+ 149.68	- δ	= + 668.9
11	+ 149.58	- δ	= + 657.3
12	+ 149.48	- δ	= + 646.0
13	+ 148.58	- δ	= + 579.9
14	+ 148.38	- δ	= + 568.5
15	+ 148.18	- δ	= + 557.8
16	+ 148.08	- δ	= + 546.6
17	+ 147.88	- δ	= + 535.3
18	+ 147.78	- δ	= + 524.4
19	+ 147.58	- δ	= + 513.3
20	+ 147.38	- δ	= + 502.3

I have treated all the equations as of equal weight in spite of the statement that the 4th and 7th were made with special accuracy, because it appeared that the practice of expressing the observed time in quarters of a day would smother the superior accuracy of any particular observations.

First Analysis of Observations.

A least squares solution gives the results—

$$s = +1''.0 \pm 0''.18, \quad \delta = -7'.6 \pm 0'.46 \quad . \quad . \quad (1)$$

The probable error in each observed declination after correcting for the constant error in declination amounts to $\pm 1'.9$. Of the constant error in declination, which it may be simpler to describe as the equator error, $-0'.7$ is explained by Hipparchus' ignorance of refraction.

But this result requires considerable modification. The effect of giving the observed times to the nearest quarter of a day is to divide the observations into groups, producing an artificial concord between observations belonging to the same group and an artificial discord between observations of the same equinox belonging to different groups. The reason for this is that the unit of time, a

quarter of a day, is nearly an exact factor of the tropical year, and in consequence the interval between two successive observations of the same equinox appears either as an exact multiple of 365 days 6 hours, or as 6 hours less than such a multiple. While the spring observations show the uniform interval of 365 days 6 hours and thus possess an artificial concord, the autumn observations disclose three leaps of 6 hours, exaggerating the discord where the leap takes place, *i.e.* between the first and second, third and fourth, fifth and sixth observations, but minimising it between the second and third, fourth and fifth observations. A least squares solution can take proper account of the discordances of the six autumn observations, but not of the artificial character of the concord of the fourteen spring observations. If there were no errors of observation, the maximum inaccuracy in each longitude of the Sun resulting from the expression of the observed time to the nearest fourth of a day would be one-eighth of the daily motion of the Sun, which at the dates of these observations would be about $437''.7$. The semi-range of the tabular longitudes of the fourteen observations is $248''$. It therefore follows that a systematic error of $189''.7$ may be lurking in these longitudes. As it is probable that Hipparchus did not always determine the time exactly to the nearest quarter of a day, a larger error is possible; but it is equally possible, I might say probable, that if some of the observed times had lain almost exactly three hours distant from noon or midnight, they would have been attributed to the wrong quarter and have disturbed the apparent concord. A separate analysis of the residuals obtained from these fourteen observations, if conducted by the ordinary method, gives $\pm 13''$ as the probable error attaching to the mean correction of declination, whereas the probable error really arising from the inexactness of the statements of the observed times should be $\pm \frac{1}{2} \sin \epsilon 189''.7$ or $\pm 38''$. It will be seen, then, that the error attaching to these observations is only very imperfectly represented by a least squares solution, and we shall not greatly exaggerate the probable error attaching to the result if we treat the probable error arising from the inexactness of the statements of time as independent of the probable error arising from the discordances. I have therefore tried the effect of introducing a constant correction of $+189''.7$ into the observed - tabular longitudes for the fourteen spring equinoxes. Solving the twenty equations of condition with this correction, I get an addition of $+0''.25$ to the value of s determined above, and of $-0'.65$ to the value of δ determined above. The probable error arising from the inexactness of stated times should be half these quantities, *i.e.* $\pm 0''.13$ in s and $\pm 0'.33$ in δ . Combining these probable errors with those deduced from the discordances, I get

$$s = +1''.0 \pm 0''.22, \delta = -7'.6 \pm 0'.56 \quad . \quad . \quad (2)$$

Comparison with Stellar Observations.

We are happily able to test this conclusion, so far as the declinations are concerned, by Hipparchus' observations of star declinations. A large number of declinations are given in an early work of Hipparchus, his *Commentary on Aratus*, while eighteen are cited by Ptolemy,* apparently from the treatise on the "Displacement of the Solstitial and Equinoctial Signs." Ptolemy treats these observations as having been made 265 years before his own, which, as he tells us elsewhere,† were mostly made in the first year of Antonine (+137 July 20 to +138 July 19). The beginning of that year was, as is well known, the epoch of Ptolemy's star catalogue.‡ I have, therefore, taken from Dr. Neugebauer's *Sternatafeln*,§ which are sufficiently accurate for our purpose, the declinations of these eighteen stars for the epoch -127.28, which is 265 Egyptian years before +137.55, the epoch of Ptolemy's catalogue. The eighteen observations show that Hipparchus' declinations require a mean correction of +0.5; but, as we are only concerned in the present investigation with the error of his equator, I have made a separate analysis of observations within 10° of the equator. I tabulate accordingly:

TABLE III.

	Hipparchus' Declination.	Declination according to Neugebauer.	Hipparchus' Error. (Tabular - Observed).
Altair . . .	+5°80	+5°68	-0°12
Aldebaran . .	+9°75	+9°70	-0°05
Bellatrix . .	+1°80	+1°79	-0°01
Betelgeuse . .	+4°33	+4°25	-0°08
Spica . . .	+0°60	+0°54	-0°06
α Libræ . . .	-5°60	-5°64	-0°04
β Libræ . . .	+0°40	+0°25	-0°15

It will be observed that in each instance Hipparchus places his equator too far to the south. Using the same nomenclature as before, I find

$$\delta = -4'4 \pm 0'74 (3)$$

where, as before, -0.7 is the error due to refraction, while the probable error of a single observation after correction of the equator is $\pm 2'0$, or almost the same as that found from the equinox observations.

The large discrepancy between the false equators found from the two series of observations is most probably explained by the supposition that Hipparchus revised his equator. As the equinox observations are spread over $33\frac{1}{2}$ years, while the star declinations

* *Ubi supra*, ii. (1903), pp. 19-23. † *Ibid.*, p. 15. ‡ *Ibid.*, p. 36.

§ *Tafeln zur astronomischen Chronologie*, i. (1912).

were observed at the very end of that range of dates, Hipparchus would not be the man we conceive him to have been, if his equator at the latter date had not been considerably better than his mean equator for the whole range of dates. With a view to obtaining light on this revision of the equator, I examined the four declinations of stars within 10° of the equator given in the *Commentary on Aratus*, computing the declinations where they were not given by Neugebauer. I found, however, that the discordances were too great to give any reliable indication of the equator to which they were referred.

Discussion of Assorted Observations.

Had the spring and autumn observations been equally distributed over the whole interval under discussion, the revision of the equator would have been unimportant. But, as it is, the mean epoch of the autumn observations is much earlier than that of the spring observations, and there is therefore every reason to suppose that there was an appreciable difference between the mean equators of the two series. I have, therefore, thought it best to make a separate analysis of observations 4-11, containing three autumn and five spring equinoxes, the two series being not unevenly distributed. The analysis yielded the results, $s = +2''.0$, $\delta = -6'.4$. The probable errors found in the usual way were $\pm 0''.18$ for s and $\pm 0'.45$ for δ .

I then tried the effect of adding to the spring declinations a constant of $154''.5$, the largest addition consistent with a uniform statement of time to the nearest fourth of a day. This added $+0''.51$ to the value of s and $-1'.31$ to the value of δ . I therefore took as probable errors due to the method of stating time half these quantities, viz. $\pm 0''.25$ and $\pm 0'.65$ for s and δ respectively. The combination of the probable errors from the two sources gives

$$s = +2''.0 \pm 0''.31, \delta = -6'.4 \pm 0'.79 \quad (4)$$

The error in Hipparchus' equator as determined from the star declinations places us in a position to determine the secular acceleration from the spring equinoxes alone for the years in the neighbourhood of the star observations. It will be observed that there is a gap of six years between observations 12 and 13, after which we have eight observations made in consecutive years, the epoch of the star observations being near the close of the series. I have, therefore, made a separate analysis of these eight observations, taking the equator correction and also its probable error from the analysis of star declinations. In this series the discordances of the original observations are smothered by the method of stating the time, and I have, therefore, not attempted to find the probable error to be deduced from the discordances. I have assigned to each observation a weight proportional to the square of the coefficient of s in the equation of condition and determined the weighted mean value of s , which comes out as $+1''.9 \pm 0''.30$. The probable error

thus obtained must be modified by the probable error arising from the method of stating the time. This time I find that the maximum constant correction to the declinations is $\pm 137''\cdot 25$. I accordingly treat half this quantity as a probable error attaching to each observed declination and find a probable error from this source of $\pm 0''\cdot 46$. The combination of the two probable errors gives as the secular acceleration resulting from observations 13-20 :

$$s = +1''\cdot 9 \pm 0''\cdot 55 \quad (5)$$

This agrees very satisfactorily with the value independently determined from observations 4-11. A combination of the two series gives the final value :—

Correction to Newcomb's secular acceleration of the Sun

$$= +1''\cdot 95 \pm 0''\cdot 27 \quad (6)$$

As Newcomb's value for the acceleration is only $-0''\cdot 02$, it is unimportant whether we call the value just determined the acceleration or the correction to the acceleration.

I append a separate determination from observations 4 and 7 for what it may be worth, though I have questioned above whether, in view of the method of stating the time, their superior accuracy is of any importance. These two observations give $s = +1''\cdot 7$, $\delta = -7'\cdot 4$. As the probable error of the mean of two observations should be twice that given in (4) as the mean of eight observations, it will be seen that these results are well within the probable error attaching to the series to which they belong.

Observation with the κρίκος.

The single observation made with the κρίκος at Alexandria is of interest because of the value attached to this instrument by Hipparchus.* The instrument was fixed in the plane of the equator with a slit so placed that the illumination from the Sun's rays would pass from one side to the other when he crossed the equator. Ptolemy† raises the objection that, if permanently fixed, it is liable to be affected by a settlement in the ground, as was proved, in his opinion, by the fact that it often showed the Sun's passage across the equator twice in one day. This phenomenon, as has been frequently remarked, may easily be explained by refraction.

Taking $29^\circ 51'$ East as the longitude of Alexandria, I compute as follows :—

Date.	Tabular Longitude.	Observed - Tabular Longitude.
- 145 Mar. 23 ^d 21 ^h 9 ^m	$359^\circ 42' 50''$	$+ 17' 10''$

With terrestrial latitude $31^\circ 11'$, hour-angle 345° , and apparent declination 0, we get $34^\circ 17'$ for the Sun's apparent zenith distance

* Ptolemy, *Math. Sym.*, i. p. 195.

† *Ibid.*, p. 197.

and a refraction of $39''\cdot7$. The error in declination due to the failure to correct for refraction will be given by the formula

$$\delta = -R \sin \phi \operatorname{cosec} z,$$

where δ is the correction to be applied to the observed declination, R is refraction, ϕ is the terrestrial latitude, and z is the apparent zenith distance. In the present instance δ will be $= -36''\cdot5$. Assuming that there is no error in the observed declination apart from refraction, taking the coefficient of s from No. 7 in Table II. above, and reducing the difference between observed and tabular longitudes to declination by the factor $\sin \epsilon \cos \odot$ as above, I get the equation

$$+150\cdot2s + 36''\cdot5 = 414''\cdot3$$

from which it follows that $s = +2''\cdot5$.

This would be the correct value, if we could be sure that the *κρίκος* was set in the true plane of the equator. But, since Hipparchus at Rhodes and Ptolemy at Alexandria both placed the equator too far to the south, there is a presumption that the *κρίκος* was misplaced in the same direction, and the result probably requires a negative correction which we have no means of determining. For this reason the observation made with the *κρίκος* cannot be used to determine the secular acceleration, but only to confirm a result obtained by other instruments whose errors can be determined.

Comparisons of Eclipsed Moon with Spica.

In addition to the series of twenty observations and the one observation made with the *κρίκος*, Hipparchus recorded a number of observations in which he had compared the position of the eclipsed Moon with Spica. Eclipses were selected near the spring equinox. The motion of the Sun between the equinox and the middle of the eclipse was taken from his theory of the Sun; the Moon was assumed to be opposite the Sun, and parallax was applied in order to obtain her apparent position.* The observed difference in longitude was supposed to give the longitude of Spica, and the fluctuations in this longitude as resulting from different observations were supposed to be a measure of the oscillation of the equinoctial point. As having led the greatest of ancient astronomers to favour a false hypothesis, these observations will always be of historical interest. Unfortunately for our present purpose, Ptolemy has cited no figures except two of the inferred longitudes of Spica and the dates of the equinoxes from which these were computed, and the two longitudes that he has selected are admitted to be the most discrepant of the series,† and are, therefore, in

* Ptolemy makes no reference to parallax in his summary of Hipparchus' method, *ubi supra*, p. 199, but he makes the suggestion a little lower that Hipparchus' parallax may have been erroneous, *ibid.*, p. 200.

† *Ibid.*, p. 198.

all probability the least accurately observed. Happily we are sufficiently acquainted with Hipparchus' theory to compute the motion of the Sun between the equinox and the middle of the eclipse, and to make an approximation to the necessary correction for parallax; combining these with the inferred longitude of Spica, we can estimate to within a few minutes the observed difference in longitude between the Moon and Spica. I have taken the times of the eclipses from Oppolzer's "Syzygientafeln,"* and have computed the Sun's longitude and Moon's parallax both from Hipparchus' theory and from Newcomb's tables combined with modern theory. The exact time is unimportant, because a small error would affect equally the lunar place as determined by Hipparchus and by modern theory. I have thought it unnecessary to print any details of the computation, because the result would hardly justify such a trespass on the Society's space. But the following results may be of interest:—

TABLE IV.

Modern Theory.											
Greenwich Mean Time.				Apparent Longitude of Spica, as affected by Refraction.		Apparent Longitude of Moon, as affected by Parallax and Refraction.		Difference in Apparent Longitude.			
		d	h	m	°	'	"	°	'	"	
1	- 145	Apr. 21	9	26	174	3	26	207	36	36	+ 33 33 10
2	- 134	Mar. 20	12	47	174	12	47	176	34	42	+ 2 21 55

Hipparchus' Theory.													
Inferred Longitude of Spica.				Apparent Longitude of Moon.		Observed Difference in Longitude.		Correction to Tabular Longitude.					
				°	'	"	°	'	"	°	'	"	
1				173	30		207	55	32	34	25	32	+ 52 22
2				174	45		176	58	36	2	13	36	- 8 19

Secular Acceleration of Sun.	
1	+ 9'0
2	- 1'3

The weighted mean of the two results for the secular acceleration of the Sun is $+3''.7$, and, if it is permissible to derive a probable error from the discordance of only two observations, the probable error according to the ordinary theory should be $\pm 3''.5$. This, however, does not take account of the fact that the two observations are the two extremes of a series; in the present case the mean ought to possess a higher value than the mean of two observations selected at haphazard, but, even so, the observations can scarcely do more than support the likelihood of the existence

* *Publication der astronomischen Gesellschaft*, 16 (1881).

of a secular acceleration of the Sun. They do not help us to determine its value.*

Ptolemy's Equinox Observations.

This completes the equinox observations cited by Ptolemy from Hipparchus. For the sake of completeness I proceed to the discussion of those of his own equinox observations which Ptolemy has recorded. One of these is recorded in the seventh chapter of the third book and two others in the first chapter of the third book of the *μαθηματικὴ σύνταξις*, from which Hipparchus' observations were cited. I quote the actual records of all three observations, though I recommend my readers to study the whole of the third book if they wish to grasp the conditions of ancient observations and their relation to the theory of the Sun's motion. The first of these observations possesses a special interest, because it was the means of that false determination of the equinox which gave rise to the curious allegation that Ptolemy's star catalogue was not authentic.

"One of the most accurate of the first equinoxes observed, by us was an autumn equinox which fell in the 17th year of Hadrian on the Egyptian 7th of Athyr two equinoctial hours approximately after noon."†

"And after 285 years, in the 3rd year of Antonine, which is the 463rd after the death of Alexander, we observed again most reliably the autumn equinox which fell on the 9th of Athyr one hour approximately after sunrise."‡

"And we find that the spring equinox, which was similarly 285 years later in the 463rd year from the death of Alexander, fell on the 7th of Pachon one hour approximately after noon."§

Computing as in Table I. above, except that I have on the present occasion introduced the largest term in nutation into the tabular longitude, I find:—

TABLE V.

	Date.	d h m	Tabular	Observed -
			Longitude.	Tabular Longitude.
1	+132 Sept. 24	23 53	181 17 25	- 1 17 25
2	+139 Sept. 25	16 50	181 17 26	- 1 17 26
3	+140 Mar. 21	23 9	0 45 14	- 0 45 14

* An opinion has recently become current that, because Hipparchus found that Spica had a longitude of 174° in his time, compared with one of 172° in the time of Timocharis (Ptolemy, *ubi supra*, ii. pp. 12, 13), he must have adopted a value of precession doing justice to this change of longitude. But in view of the discordances in his own observations of Spica, it is unlikely that Hipparchus drew any such conclusion, and there is no evidence that he ever attempted anything more precise than his minimum value of $36''$ per annum.

† *Math. Syn.*, ed. Heiberg, i. p. 256.

‡ *Ibid.*, p. 204.

§ *Ibid.*, pp. 204, 205.

I then proceed as in the case of Hipparchus' observations, substituting $+0.0349$ for $+0.0351$ as the coefficient of $\cos g$ in the formula for the correction to the Sun's declination due to secular acceleration. This substitution is necessitated by the diminution in the eccentricity of the Earth's orbit between the epochs of Hipparchus' and Ptolemy's observations. As I have chosen to introduce nutation, the formula for obliquity has received the addition of the term $+9'' \cos \Omega$, where Ω is the longitude of the Moon's ascending node. I thus form the following table:—

TABLE VI.

1	-113.08	$-\delta$	$= +1865''$
2	-112.18	$-\delta$	$= +1865$
3	$+109.48$	$-\delta$	$= -1090$

These give the result

$$s = -13''.3 \pm 0''.035, \quad \delta = -6' 7'' \pm 3''.9 \quad . \quad (7)$$

The smallness of the probable error attaching to a result so greatly at variance with those derived from other observations is doubtless due to two causes. (1) The observations are so few in number that an accidental concord can easily affect the apparent probable error. (2) The observations are selected for their accuracy, *i.e.* in all probability for their agreement with Hipparchus' tabular places, and hence show a surprising agreement in favour of a false result.

Comparison with Stellar Observations.

This will become more apparent when we compare the declination error with that found from the star declinations. I have compared Ptolemy's star declinations as given in the third chapter of the seventh book of the *μαθηματικὴ σύνταξις*,* with the declinations in Neugebauer's tables for $+137.55$, the epoch of Ptolemy's catalogue. I find that Ptolemy's eighteen declinations require a mean correction of $-2'.4$. As before, I tabulate separately the observations within 10° of the equator.

TABLE VII.

	Ptolemy's Declination.	Declination according to Neugebauer.	Ptolemy's Error. (Tabular - Observed.)
Altair . .	$+5.83$	$+5.79$	-0.04
Bellatrix . .	$+2.50$	$+2.65$	$+0.15$
Betelguese . .	$+5.25$	$+4.95$	-0.30
Spica . .	-0.50	-0.94	-0.44
α Libræ . .	-7.17	-7.07	$+0.10$
β Libræ . .	-1.00	-1.11	-0.11

* *Ubi supra*, ii. pp. 19-23.

These give the result $\delta = -6'.5 \pm 3'.8$, while the probable error of a single observation after correction of the equator comes out as $\pm 9'.3$, or nearly five times as much as that of a similar observation by Hipparchus. The discordances in the star declinations are clear proof that the agreement of the equinox observations is either fortuitous or artificial. I have thought it worth while to work up the equinox and stellar declinations as a single series of observations, allowing double weight to the equinox observations as being the mean of two observations in each case. The result then becomes

$$s = -13''.3 \pm 1''.8, \delta = -6'.3 \pm 2'.3 \quad . \quad . \quad (8)$$

From a comparison of the probable error attaching to s in this equation with that found in (6), it would appear that even if Ptolemy's observations had not been selected on the ground of their accord with Hipparchus' tables, the secular acceleration deduced from Hipparchus' observations would be entitled to a weight forty-three times as great as that deduced from Ptolemy's observations. The latter can hardly be used to modify the value given in (6).

The Equinox of Ptolemy's Star Catalogue.

Of the three equinox observations just discussed the first is that from which Ptolemy deduced the epoch of the Sun's longitude. From Hipparchus' theory of the Sun he finds that at the autumn equinox the equation of the centre should be $-2^\circ 10'$, and therefore makes the mean longitude of the Sun at the date of this observation $182^\circ 10'$. Newcomb's tables give for the mean longitude unaffected by aberration $183^\circ 9' 31''$. The correction for secular acceleration found in (6) is $+9' 2''$, so that the corrected mean longitude should be $183^\circ 18' 33''$, and the correction required to reduce Ptolemy's mean longitude to the true mean longitude is $+1^\circ 8' 5''$. Hipparchus' mean motion of the Sun in a Julian year is $360^\circ 0' 12''$. At the epoch in question the true value was $360^\circ 0' 26''$. Hipparchus' error is therefore $+14''$ per annum. In 482 years, the interval between the observation and the epoch of the star catalogue, the error in the mean motion would amount to $+1'.2$, so that the error in the deduced mean longitude at the epoch of the star catalogue would be $+1^\circ 9' 7''$. Now Mr. Knobel has found* that the mean error of Ptolemy's longitudes of zodiacal stars, when compared with the true longitudes for +100, is $+34'.9$. 3755 years precession at $49''.9$ per annum amounts to $+31'.2$, so that the mean error in the star longitudes for +13755, the epoch of the star catalogue, is $+1^\circ 6' 1''$. It is clear, therefore, that the false equinox in the catalogue reproduces to within a few minutes the false equinox as obtained five years

* C. H. F. Peters and Knobel, *Ptolemy's Catalogue of Stars: a Revision of the Almagest*, Carnegie Institution of Washington, 86, 15, 17 (1915).

earlier from the erroneous equinox observation from which Ptolemy professes to derive the epoch of the Sun's motion. As the star places are professedly derived from the solar places, it is curious that the fable which accuses Ptolemy of having copied his star catalogue from Hipparchus, merely adding a false precession, should ever have gained currency. It was amply refuted by Laplace, *Exposition du système du monde*, 5^{me} éd. (1824), ii. pp. 293-296, who gave the correct explanation of the false equinox.

The question still remains how Ptolemy came to make such an erroneous observation of the equinox. The error in declination amounts to $-35'$, whereas the largest error in his star declinations, that of Benetnasch, is approximately $-28'$. The error in mean longitude appears also to be greater than that which would arise from the use of Hipparchus' tables with their error in the mean motion of the Sun. A study of Hipparchus' tabular longitudes as cited by Ptolemy might throw some light on the question. For the present we can only suppose that Ptolemy determined his epoch of the Sun's longitude from a particularly bad observation, which he regarded as good because it seemed to agree fairly well with Hipparchus' tabular longitude; the good observations would naturally differ far less. It may also have been the case that the Sun observations, being made with a different instrument from the star observations, were affected by a larger equator error, but this could only account for a small part of the total error.

Application of the Result to the Motion of the Moon's Node.

In *Monthly Notices*, 75, 396 (1915), I found by a least squares analysis of the corrections to Dr. Cowell's acceleration of the Sun's distance from the Moon's node, as given in my paper (*M.N.*, 69, 668) (1909), that the theoretical acceleration of the Sun measured from the node required an empirical correction of $+1''\cdot9 \pm 0''\cdot45$. More exactly this might have been expressed as $+1''\cdot94 \pm 0''\cdot45$. Dr. Brown's latest value for the longitude of the node, as given in *Monthly Notices*, 75, 510 (1915), yields the following correction to Dr. Cowell's longitude of the node used in my paper,

$$+6''\cdot86 - 5''\cdot17 T - 0''\cdot14 T^2 + 0''\cdot001 T^3,$$

where T is reckoned in Julian centuries from 1800 Jan. 0.0.

This correction, reduced to -29 , the mean epoch of the observations on which my investigation was based, is equivalent to $+48''\cdot48$, to compensate which it will be necessary to apply to the resultant acceleration of the Sun measured from the node a correction of $-48''\cdot48/(-18\cdot29)^2$, i.e. of $-0''\cdot14$, reducing the empirical correction to be applied to the acceleration of the Sun measured from the node to $+1''\cdot80 \pm 0''\cdot45$. But in (6) it was found that the correction to the secular acceleration of the Sun was $+1''\cdot95 \pm 0''\cdot27$.

From these two equations it follows that the empirical correction to be applied to the secular acceleration of the node is $+0''.15 \pm 0''.53$. But it is open to us, if we choose, instead of correcting the acceleration of the node to correct its centennial motion at the epoch 1800 Jan. 0.0. To do this we shall have to multiply the correction just found by -18.29 , and we thus obtain a correction of $-2''.8$ to Dr. Brown's observed centennial motion at epoch. The effect of this is to remove the small outstanding difference between theory and observation in the motion of the node within the range of modern observations. It would appear, therefore, that the ancient observations are best represented by adopting the theoretical motion and theoretical acceleration of the Moon's node without change. This close agreement is, of course, in a certain measure accidental. The difference between Dr. Brown's observed and theoretical motions of the node is well within the probable error attaching to the result of my investigation. I cannot correct Dr. Brown's figures; I can only show that a similar agreement between theory and observation has held good for the last two thousand years.

Comparison with Previous Results.

I have not re-investigated any of the evidence furnished by eclipses where either the secular acceleration of the Sun or that of the Moon's node produces an effect in combination with the secular acceleration of the Moon's mean motion, but I note in passing that the value now proposed for the secular acceleration of the Sun when combined with the value $+10''.8$, found by Miss Longbottom and myself for the secular acceleration of the Moon, will just fit the diagram which I published in *Monthly Notices*, 69, 210 (1909), showing values for the acceleration of Sun and Moon respectively, which will satisfy the eclipse of Hipparchus.

It will be observed that the new value for the secular acceleration of the Sun is $+1''.25$ less than the value which I found in 1915 (*Monthly Notices*, 75, 396), as the result of a comparison of ancient occultations and conjunctions with ancient lunar eclipses. As this is only one and a half times the probable error attaching to that result, I cannot regard myself as unlucky. The new result comes out with only one-third the former probable error, and has the additional advantage of satisfying the theoretical motion of the node.

I repeat my conclusion :

Secular acceleration of Sun = $+1''.93 \pm 0''.27$ per century.

6 Blackhall Road, Oxford :
1918 March.

On the Early History of the Solar System.

By Harold Jeffreys, M.A., D.Sc.

I. *Introduction.*

The only explanation of the formation of mountains that has yet been advanced is that in some way the interior of the earth has contracted and thereby thrown the crust into a state of compression. The deposition of solid matter on the outside would not produce such an effect, for the last layer deposited would necessarily be in a state of zero strain, and hence, whatever stresses occurred within, the outer surface would be unstrained and would therefore not be distorted. Now, when the earth is assumed to have formerly been fluid, and the available data with regard to its age and thermal condition are used, it is found that the available compression produced by thermal contraction is of the same order of magnitude as that required to account for mountain-building.* On the same hypothesis many other facts relating to the present condition of the earth and moon can be correlated.† There is, therefore, a strong probability that the earth was formerly fluid. Again, the four great planets are now probably gaseous to a considerable extent; from this statement it is but a short step to the hypothesis that all the planets were formerly either liquid or gaseous.

We next inquire what was the condition before these large masses took form. Supposing them to have been produced gradually by condensation of a tenuous medium, we proceed to examine the conditions necessary for such condensation. The influence of viscosity in a gas can be important only when the relative mass velocities are small compared with the molecular velocities, which can never have been true in the solar system.‡ Hence, however the mass was distributed, the motions must have been controlled principally by gravity, and the velocity at any point must have been of the same order of magnitude as that of a planet moving in a circular orbit through the same point. Hence ω , the angular velocity of the matter at this point, must have been of the same order as n , the mean motion of this ideal planet. If, for instance, M denote the mass of the sun, and f the constant of gravitation, the velocity at distance r from the centre is of order rn , or $(fM/r)^{\frac{1}{2}}$. The rotation of the matter at this distance is of order $\frac{1}{r} \frac{\partial}{\partial r}(r^2 n)$, which is of order $\frac{1}{2}n$. Now matter can only condense if its density ρ is greater than some quantity of order $\omega^2/2\pi f$, or practically $n^2/8\pi f$. Substituting for n^2 , we have the

* *Phil. Mag.*, 32, 575-591 (1916).† Sir G. H. Darwin, *Scientific Papers*, 2; A. Holmes, *Geological Magazine*, February-March 1915, June 1916; Jeffreys, *Memoirs R.A.S.*, 60, 187-317 (1915); *Monthly Notices*, 77, 449-456 (1917); 78, 116-131 (1917).‡ *Monthly Notices*, 77, 203 (1917).

inequality that ρ is at least of order $M/8\pi r^3$. Thus the density where condensation occurred must have been of the order of $\frac{1}{8}$ of the density that would be obtained if the whole mass of the sun were distributed uniformly throughout a sphere of radius r . Now it is not likely that the density of the mass before condensation started decreased greatly towards the sun, and it is therefore necessary that the sun was distended to fill the orbit of each planet when this was formed. The moment of inertia of such a mass would be of order Mr^2 , and its angular momentum Mr^2n at least, for the inner parts would be revolving more rapidly than the outer ones. Hence a lower limit to the primitive angular momentum of the solar system is obtained. Now this cannot have changed with time, and therefore we obtain a lower limit to the present total angular momentum. The actual angular momentum is only about $\frac{1}{2000}$ of the limit thus fixed by the conditions for the formation of Neptune; hence the condition of the solar system contradicts the hypothesis that the planets were formed by gradual condensation.

It was thought by Poincaré* that the condition that angular momentum is constant could be complied with if all but about $\frac{1}{600}$ of the mass of the primitive nebula were concentrated in the sun; it also appears at first sight as if the fact that the constituent parts would practically describe independent orbits under gravity, instead of the whole rotating like a rigid body, as Roche assumed, would enable it to be satisfied. The present argument is a modification of that of Jeans,† and shows that if such a concentration existed the mass elsewhere would be too tenuous to condense to form planets.

The formation of planets from a symmetrical gaseous mass is, therefore, impossible; a highly heterogeneous one, with well-marked condensations initially, might lead to a system not unlike ours, but it is difficult to see what this could have been at a still earlier stage of condensation except a fairly symmetrical mass, and then the same objections would apply. It is, then, very unlikely that the planets were formed by any process of gradual evolution, and therefore it appears that they were strongly condensed from the start and were formed catastrophically.

The strongest feature of the tidal theory of the origin of the solar system is that it provides for such sudden formation of the planets. According to this theory a star of mass several times greater than that of the sun approached it so closely that its tidal action distorted the sun more than was consistent with stability, and either one or two streams of matter were shot out with considerable velocity. These streams were longitudinally unstable, and broke up almost at once into a series of fluid masses. This theory forms part of the Planetesimal Hypothesis of Chamberlin and Moulton; its dynamical possibility has been proved by Jeans ‡;

* *Leçons sur les Hypothèses Cosmogoniques*, p. 20.

† *Monthly Notices*, 77, 186-194 (1917).

‡ *Memoirs R. A. S.*, 62, pt. 1 (1917).

and I have shown here and elsewhere * that the system it would lead to would resemble our own in several striking features. It will be definitely adopted as a postulate in the present paper.

II. *The Primitive Resisting Medium.*

It is not likely that the planetary nuclei included the whole of the matter expelled, for it would be in a gaseous state and much of it would be scattered by diffusion; and some of it would be unable to condense through lack of gravitative power. Thus a quantity of matter would be dispersed through the system, and would afterwards offer a frictional resistance to the motion of the planets. Now the eccentricities of the planetary orbits would originally be great, and it is known that the effect of a resisting medium is in general to reduce them. It is to this action that the smallness of the present eccentricities must be attributed. If the medium were too dense, or acted for too long a time, the eccentricities would be less than they are; from the fact that they are small, but sensible, a rough estimate of the density of the medium and the time it continued to exist may be obtained. Its precise effect would depend largely on the distribution of density within it, which is very uncertain; as it would be composed of matter too light to be included in the nuclei, we should expect it to consist mostly of hydrogen and other substances of low molecular weight.

A mass of this type would degenerate for two reasons. In the first place, the inner parts would be moving faster than the outer, and a viscous action would take place between them. This would reduce the orbital momentum of the inner parts and make them approach the sun; at the same time the outer parts would recede. If ρ , μ , l , and τ be of the order of magnitude of the density, true viscosity, linear dimensions, and time of degeneration through viscosity, we have $\tau = \rho l^2 / \mu$. Secondly, the parabolic velocity at a distance r from the sun is $(2/r)^{1/2}$ in astronomical units; the temperature at that distance would not be very different from the equilibrium temperature of a black body at the same place; this is proportional to $r^{-1/4}$, and hence C , the velocity of agitation of the molecules, would be proportional to $r^{-1/4}$. Hence at a sufficiently great distance from the sun most of the molecular velocities would exceed the parabolic velocity, and the gas would therefore escape at once into space. Supposing the solar radiation not to have changed greatly, I estimate this distance to be about half a parsec. On the other hand, if the time needed for escape were of the order of 10^7 years, a short time cosmogonically, the molecular velocity need only be about $\frac{1}{6}$ of the parabolic velocity, and this would be attained at a distance a few times greater than that of Neptune. Thus there has been ample time for the outer parts of the resisting medium to escape into space. Viscosity and diffusion, on the other hand, would cause matter to be supplied to the outer regions at

* *Science Progress*, July 1917, pp. 52-62.

such a rate as would keep pace with the loss, and consequently in time the medium would be likely to disappear wholly, part of it being reabsorbed into the sun and part lost into space.

An essential feature of the theory is the action of the medium in reducing the eccentricities of planetary orbits, and we must note that it continued to exist for a time long enough to reduce them considerably without making them insensible. Hence the time of degeneration was of the same order as the time needed to reduce the eccentricity of a planet to e^{-1} of its original value. To estimate this we must have an idea of the dimensions of the problem. When the relative velocity of a body moving in a fluid is sufficiently small, the resistance per unit area is proportional to the relative velocity, but when it exceeds a certain value dependent on the linear dimensions and the kinematic viscosity, the resistance becomes proportional to the square of the velocity. If the velocity is comparable with or considerably exceeds the velocity of sound, the square law is still a moderate first approximation, the constant coefficient being about 0.4. Now the velocity of sound is independent of the pressure, being a function of the temperature alone, and the order of magnitude of this can hardly have changed. Thus the velocity of sound in the neighbourhood of the earth's orbit was practically the known velocity of sound in hydrogen, namely, about 1 km./sec., and the orbital velocity was much more than this, even the variations in it due to the present eccentricity being of the same order. Hence the square law probably held for the motions of the planets. Now, as a first approximation, we may say that the resistance per unit cross-section to these motions was $0.4\rho U^2$, where U is the relative velocity, and therefore the time needed to reduce this relative motion to e^{-1} of its original value was of order $M/0.4\pi\rho a^2U$, M being the mass of the planet considered. Applying the theory to Mercury, for instance, this planet being the least affected by the uncertain factor introduced by the condensation around it caused by gravity,* this time is about $800/\rho$ seconds. This must be somewhat greater than the time of degeneration of the whole mass through viscosity, for the medium would last longest near Mercury. Thus putting $l = 6 \times 10^{13}$ cm., and $\mu = 10^{-4}$ gm./cm. $\times$ sec. (μ being independent of the density), we find that $800/\rho$ is greater than $4 \times 10^{31}\rho$, but not much greater. Taking the ratio to be 4, say, this gives $\rho = 2.3 \times 10^{-15}$ gm./cm.³, and the time needed to reduce the eccentricity of Mercury = 10^{17} secs. = 3×10^9 years. This gives an estimate of the age of the solar system on the tidal hypothesis; † there are of course many uncertainties in the data, the chief being in the primitive distribution of mass, but it is interesting to note that the age obtained is of the same order of magnitude

* See later, p. 431.

† Incidentally the fact that the time of degeneration through viscosity is very long compared with the periods of revolution of the planets strongly confirms the statement in the Introduction that viscosity is insensible compared with gravity in determining the relative movements of the parts of a nebula.

as the age of the earth derived by radio-active methods. At the same time, assuming a spherical distribution, we find the mass within the orbit of Jupiter to have been about 4×10^{27} grams, about two-thirds of the mass of the earth, and much less than that of Jupiter. It follows that before the resistance had considerably affected the angular momenta (and hence the major axes) of the great planets the reaction of the planets on the medium would have forced the gas to rotate in such a way that there was no relative motion in longitude. The effect on the eccentricities is likely to have been more important, for the radial motion of Jupiter, for instance, would impose a forced vibration on the gas, which would need a considerable supply of energy to maintain it except in the unlikely event of there being a free vibration of exactly the same period; this would be supplied from the energy of the radial motion of the planet; thus the eccentricities would be reduced.

In subsequent investigations it will be necessary to know whether a medium of the extreme tenuity here indicated could behave like a gas. The condition for this is that the mean free path of a molecule shall be considerably less than the circumference of the orbit about the sun; in other words, it must undergo many collisions in going round the sun. Now the mean length of the free path of a molecule of hydrogen at N.T.P. is 1.83×10^{-5} cm.* and is inversely proportional to the density. Thus in the medium considered it would be 3.6 kilometres, much less than the linear dimensions of the mass; hence the kinetic theory will be valid.

It is now possible to make certain deductions about the motion of the medium. It is supposed to be symmetrical about the axis of z ; radial and axial motion and viscosity are, in the first instance, neglected.

Taking rectangular co-ordinates x , y , and z , put $x^2 + y^2 = \varpi^2$, and let the velocity at (x, y, z) be $\omega \varpi$ perpendicular to the meridian plane; the other components of velocity are zero by hypothesis. At present ω is quite unknown. Then the equations of motion reduce to

$$\left. \begin{aligned} \frac{\partial U}{\partial x} - \frac{1}{\rho} \frac{\partial p}{\partial x} &= -\omega^2 x \\ \frac{\partial U}{\partial y} - \frac{1}{\rho} \frac{\partial p}{\partial y} &= -\omega^2 y \\ \frac{\partial U}{\partial z} - \frac{1}{\rho} \frac{\partial p}{\partial z} &= 0, \end{aligned} \right\} \quad . \quad . \quad . \quad (1)$$

where U is the gravitation potential; giving in differential notation

$$dU - \frac{1}{\rho} dp = -\frac{1}{2} \omega^2 d\varpi^2 \quad . \quad . \quad . \quad (2)$$

* Kaye and Laby, *Physical and Chemical Constants*, p. 33.

If V be the absolute temperature, we have

$$p = R\rho V$$

where R is a constant (the composition being supposed invariable). Thus

$$\frac{dU}{V} + \frac{1}{2} \frac{\omega^2}{V} d\varpi^2 = R \frac{dV}{V} + R \frac{d\rho}{\rho} \quad . \quad . \quad . \quad (3)$$

Now $U = \mu/r$, where μ depends only on the mass of the central body. The temperature is determined by the condition that each particle radiates as much heat as it receives. Radiation from other parts of the envelope being neglected in comparison with that from the sun, the heat received is proportional to $1/r^2$; that radiated is proportional to V^4 . Hence V is proportional to $r^{-1/2}$. Put then

$$V = ar^{-1/2} \quad . \quad . \quad . \quad (4)$$

Then dU/V , RdV/V , and $Rd\rho/\rho$ are perfect differentials. Therefore $\frac{1}{2}\omega^2 d\varpi^2/V$ is a perfect differential. Hence

$$\omega^2 = r^{-1/2} f^2 \quad . \quad . \quad . \quad (5)$$

where f is an arbitrary function of ϖ .

Substituting in (3) we find

$$R \log \rho = \frac{2\mu}{a} r^{-1/2} + \frac{1}{a} \int f^2 \varpi d\varpi + \frac{1}{2} R \log r + \text{constant} \quad . \quad (6)$$

The problem is therefore solved when f is known.

All the terms on the right are essentially positive. Therefore when r is great enough ρ will increase with r at least as fast as $r^{1/2}$, so that the total mass is infinite. Consequently an exact steady motion is impossible. The matter will necessarily flow outwards to some extent. This will, however, not be important unless r is so great that $\frac{2\mu r^{-1/2}}{aR}$ is small; this makes r greater than 10^{19} cm.,

or 6×10^5 astronomical units. Such outward flow is therefore not important in masses of the size of the solar system, for in them the variation of the terms with a in the denominator dominates everything. If $\omega^2 < \lambda^2 \mu \varpi^{-3}$ on the equatorial plane, where λ is a constant less than unity, however near to unity it may be, the density will diminish outwards; if $\omega^2 > \mu \varpi^{-3}$ it will increase outwards. Thus the distribution of density depends chiefly on the closeness of the approximation of the velocity of each part of the medium to that of a particle moving in a circular orbit at the same distance, and is practically arbitrary when this approximation is sufficiently close.

This solution will break down if the temperature decreases outwards faster than it would if the equilibrium were convective. Now convective equilibrium gives

$$p - k\rho^\gamma = R\rho V \quad . \quad . \quad . \quad (7)$$

where k is a constant, and

$$\frac{dp}{\rho} - dU = \frac{1}{2}\omega^2 d\varpi^2. \quad (8)$$

Hence ω is a function of ϖ , and

$$RdV = dU + \omega^2 \varpi d\varpi \quad (9)$$

If the velocity is greater than the circular velocity, the temperature in convective equilibrium will therefore increase outwards, and radiative equilibrium will be unstable; if, on the other hand, it is never greater than a certain constant proper fraction λ of the circular velocity, the temperature gradient will be negative and greater than is indicated by the formula

$$\frac{dV}{dr} = -\frac{\mu(1-\lambda^2)}{r^2 R} \quad (10)$$

while in radiative equilibrium

$$\frac{dV}{dr} = -\frac{a}{2r^{\frac{3}{2}}} \quad (11)$$

The former is numerically the greatest provided $\frac{2\mu(1-\lambda^2)r^-}{aR}$ is greater than unity, which is true provided r is not more than a quantity of the order of 10^5 astronomical units, unless λ is extraordinarily near to unity. Hence radiative equilibrium is normally stable.

In the neighbourhood of the orbit of a great planet the velocity is constrained to be nearly equal to the circular velocity. Thus near Jupiter, for instance, the velocity would be greater than near Saturn; hence momentum would be continually passing outwards; this would, however, be replaced as fast as it was removed, on account of the reaction of Jupiter on the medium. Thus Jupiter would be losing momentum, and would therefore approach the sun. Similarly Saturn would recede. This mutual influence depends on the true viscosity and not on the density, and would therefore continue until the medium became so tenuous that it ceased to behave as a gas. It is possible that this may cast some light on the relative distances of the planets and satellites.

It has already been stated that the resistance to the motion of a planet is approximately $0.4\pi\rho a^2 U^2$, where a is its radius and U its velocity relative to the medium. Hence its acceleration is $-0.4\pi\rho a^2 U^2/M$; the times needed to produce proportional effects on the relative velocities of different planets are therefore proportional to Ua^2/M , and if the densities are the same this is practically proportional to U/a . Hence in general the effect of resistance is greater the smaller the body, and the smaller planets would therefore be expected to have, on the whole, smaller eccentricities than the larger ones. This is not confirmed by observation, the largest

eccentricities in the solar system, being those of the smallest members, Mercury, Mars, and some of the asteroids.

It seems, however, that this argument would have to be seriously modified if the gravitation of the moving body were taken into account. This would cause a strong condensation around the body and moving with it, and it seems likely that the radius of this would be proportional to the mass of the planet, as the surfaces of equal density would approximately follow those of equal potential, which satisfy this condition. Now the resistance of the medium would be proportional to the square of the dimensions of the *moving* matter, and therefore to the square of the mass; thus the accelerations of large planets (the relative velocity being the same) would be proportional to their masses. Hence the effect on the eccentricity would be least for the smallest planet with such a condensation round it. To determine the limiting condition for this is likely to be extraordinarily difficult, as the mathematical investigation of the problem of even a non-gravitating body moving with a velocity greater than that of sound has hitherto proved unsuccessful; it may be conjectured, however, that it is that the velocity relative to the medium shall not be much greater than the velocity of escape from the surface of the planet. These ratios are for Mercury, 15; Venus, 3; Earth, 2.5; Mars, 4; Jupiter, 0.21; Saturn, 0.27; Uranus, 0.32; Neptune, 0.25; the medium being supposed in a state of rest. These numbers would, of course, have to be reduced if the rotation of the medium were taken into account. Thus the condensation would be expected to be important for all the major planets except Mercury, and dominant for the four great planets. So far as the effects on the eccentricities are concerned this theory agrees with the facts as well as can be expected.

The effect of a resisting medium on a satellite also requires investigation. Suppose, for simplicity, that the orbit of the primary is circular, and take rotating axes with the primary as origin, so that the sun is always on the negative part of the axis of x . Then the components of the resistance to the motion of the satellite, supposing the planet not to affect the rotation of the medium, are proportional to $\dot{x}, \dot{y} + \frac{3}{2}n'x$; n' being the mean motion of the primary.* The secular effect on the mean distance of the satellite will depend on the average value of the component of the resistance perpendicular to the radius vector joining it to the primary; this is practically proportional to $c(n + \frac{3}{4}n')$, where c is the mean distance of the satellite and n its mean motion. Hence, if there were two satellites at the same distance, one retrograde and the other direct, the numerical value of the resistance to the direct one would be the greater. Further, the tangential force would have the same sign as n , unless n were as small as $\frac{3}{4}n'$, which is not approached by even the most distant satellites. Hence we should expect both retrograde and direct satellites to approach

* *Monthly Notices*, vol. 77, 106.

their primaries, though direct ones would do so somewhat more rapidly. The difference would not be enough to account for the very great distances of the retrograde satellites, which probably correspond to a fundamental difference of origin.*

We must next investigate whether the effect of a resisting medium would be great enough to counterbalance tidal friction. The acceleration it produces is practically $-1.3 \rho a^2 c^2 n^2 / M$. If M_0 be the mass of the primary, a_0 its radius, and g the value of surface gravity, the height of the equilibrium tide is of order $f M a_0^2 / g c^3$, and the couple it produces on account of its attraction on the satellite is of order $\left(\frac{f M}{c^3} \right)^2 \frac{\rho_0 a_0^6}{g} \sin 2\epsilon$, ρ_0 being the density of the primary and ϵ the lag of the tide. Put $M/M_0 = \kappa$. Then $f M / c^3 = \kappa n^2$, and the couple is of order $\kappa^2 n^4 \rho_0 a_0^6 \sin 2\epsilon / g$. Hence the acceleration is of order $\kappa^2 n^4 \rho_0 a_0^6 \sin 2\epsilon / g M c$. Thus the ratio of the effect of the resisting medium to that of tidal friction is of order $\frac{\rho}{\rho_0} \frac{a^2 c^3 g}{\kappa^2 n^2 a_0^6 \sin 2\epsilon}$ or $\frac{\rho}{\rho_0} \frac{a^2 c^6}{\kappa^2 a_0^8 \sin 2\epsilon}$. The lag of the tide being the same, it follows that the resisting medium is relatively most effective when the distance is great. In the case of the earth and moon, the mean distance being supposed to have its present value, and ρ being put equal to 2.3×10^{-15} gm./cm.³, this ratio is found to be about $.02 \cos 2\epsilon$. Thus at a certain stage of the evolution, after the plasticity of the earth had passed its optimum value, the two agencies may have been comparable. The chance that the general course of the evolution can have been upset by resistance is, however, small. On the other hand, for a very small and remote body like the eighth satellite of Jupiter, the resistance is much more important than tidal friction. For the fourth satellite again, and for the satellites of Mars, I estimate the ratio to be of the order of $\text{cosec } 2\epsilon$. It is, therefore, quite likely that in all the subsystems except our own the effect of resistance has been preponderant over tidal friction, at least while the primaries have had their present dimensions. This agrees with the insensible eccentricities of the satellites of Mars, the two innermost of the great satellites of Jupiter, the five innermost satellites of Saturn, and all those of Uranus and Neptune.† At the same time the smallness of the amplitude of the libration of the three inner great satellites of Jupiter requires an explanation. I hope to investigate soon whether these facts agree better with the theory of tidal friction or with that of a resisting medium.‡

* If the condensation around a planet caused the medium in its neighbourhood to rotate much more rapidly than in the absence of the planet, this statement would need to be modified.

† Young, *General Astronomy*, p. 605.

‡ When the period of revolution of a satellite is longer than about one and a half times that of the rotation of the primary, tidal friction will cause the eccentricity to diminish if the elliptic monthly tide lags more than the short-period tides; otherwise it will increase. If the primary is nearly perfectly elastic, firmo-viscosity will make the eccentricity increase, while elastico-viscosity will make it diminish.

III. *The Physical State of the Primitive Planets.*

As has already been stated, there is a strong probability that the earth was formerly liquid, and that the four great planets are still largely gaseous. Now if a small mass of gas were let loose in a vacuum, it would at once spread out by diffusion till the whole space was homogeneous; on the other hand, large masses, such as gaseous stars, can exist for long periods without serious loss, the mutual attractions of their parts being more than enough to counteract the tendency to spread out. It may be asked what is the limit of mass below which a gaseous body is unable to hold together. Evidently when it is in a vacuum the condition is that the velocity of agitation of the molecules at the surface shall be much less than the velocity of escape. For, let v be the velocity of an individual molecule at the surface of a gaseous planet of mass M and radius a . Then it will be able to recede beyond the influence of gravity if $\frac{1}{2}v^2 > fM/a$, where f is the constant of gravitation. If C be the mean molecular velocity for the molecular weight and temperature considered, the condition will practically hold when $C^2 < \frac{1}{10}fM/a$,* making the life of the body of the order of 10^8 years. Now C , for hydrogen at 273° absolute, is 1.84×10^5 cm./sec., and hence for water vapour, whose molecular weight is nine times that of hydrogen, the value of C at 1500° absolute is 1.4×10^5 cm./sec. The absolute temperature is not likely to have been less than this, and hence the greatest possible radius is $\frac{1}{10}fM/C^2$, or practically $fM/2 \times 10^{11}$, where C.G.S. units must now be used. For the earth this gives a radius of 2×10^9 cms., about three times its present value, and for other planets it is proportional to the mass.

This theory will not, however, hold for the primitive planets, for these were by hypothesis surrounded by a gaseous medium and not by a vacuum. Hence a molecule on escaping could only describe a free path of some 3 kilometres on an average before striking a molecule of the medium and having its velocity reduced to less than the velocity of escape. The loss of mass due to the failure of the kinetic theory at the edge of a vacuum would not occur in the solar system as it originally was, provided only that the nuclei were massive enough to be able to exist in the first place.

Now if p be the pressure, ρ the density, and V the temperature, we have

$$dp = \rho dU,$$

giving for an isothermal mass,

$$U = \text{constant} + RV \log \rho.$$

Near the boundary of such a mass we have $U = fM/r$, and hence ρ diminishes like $e^{fM/RVr}$. For hydrogen R † is about 4×10^7 ,

* Jeans, *Dynamical Theory of Gases*, 1st edition, p. 323.

† C.G.S. centigrade absolute units are always used unless the contrary is explicitly stated.

and for the earth, supposed at 1500° absolute, fM/RV is about 6×10^9 cms. Thus a change in r from 10^9 cms. to 1.2×10^9 cms. would make a very great drop in the density. The expression "the boundary of a planet" therefore has a sufficiently definite meaning.

Suppose the whole planet sufficiently distended for the gas laws to apply approximately throughout it. Then the pressure at the centre is $\int \rho dU$, which is of order $\rho fM/a$, ρ being now the mean density. The central pressure, on the other hand, satisfies

$$p = RV\rho,$$

and provided V and ρ are of the same order of magnitude throughout most of the mass, which is probable, this shows that fM and aRV are of the same order. Thus a is of order fM/RV .

Now R is inversely proportional to the molecular weight, and therefore for silica (m.w. = 60) it is 1.3×10^6 . For the earth a is about 1.8×10^{11} cm., and an estimate of the distension of the primitive earth, assumed wholly gaseous, is therefore obtained. Further, the mass of a solid planet of radius a_0 is about $12a_0^3$. Now for any planet the radius when gaseous is greater than the radius after it solidified. Hence

$$12fa_0^3/RV \text{ is greater than } a_0.$$

Supposing the planet to have been composed of silica vapour, this shows that a_0 is greater than 5×10^7 cm.; thus a planet originally gaseous cannot have a smaller diameter than 1000 km.

This condition is satisfied by all the major planets, the moon, four of the satellites of Jupiter, and probably by eight satellites of Saturn and all those of Uranus and Neptune, though in several cases the margin of safety is very narrow. It is certainly violated by all the asteroids except perhaps Ceres, and by all the other satellites. It depends only on the substance the planet is composed of, and is independent of the surroundings. It follows that all the bodies whose diameters are less than 1000 km. must have been liquid or solid from the commencement.

On the tidal theory there is nothing very surprising in this result. A mass expelled from the sun would be initially gaseous, but on being released from the high pressures that must exist even near the surface of the sun it would expand adiabatically and therefore cool, the minimum temperature reached being the boiling-point. The smallest declines in pressure, and therefore in temperature, would probably take place in the largest planets, which might remain gaseous. The smaller bodies would cool down enough to make most of their substance liquefy, the drops possibly collecting subsequently. The uncondensed matter, with those liquid particles that passed beyond the control of their primaries, would be dispersed through the system to form the resisting medium. The liquid particles and the smaller satellites would solidify almost at once. If the former were plentiful their

mutual collisions would soon transform them again into gas; otherwise they would resemble meteors.

While this explanation may be satisfactory for the satellites, there remains a difficulty with regard to the asteroids: namely, why are all but one of them between the orbits of Mars and Jupiter? If they were originally independent, we should expect a much wider range among their major axes, and in particular it is remarkable that not one is known to move between Jupiter and Saturn. This uniformity implies a closer connection between their origins than the relations between other planets; and it is suggested that they were formed from some larger body after it had attained the liquid or even solid state.

The next question is, what could break up such a body into more than 800 smaller bodies? The rupture could be produced either by wholly internal or by partly external agencies. Of the former class only two seem conceivable: explosion, for the possibility of which there is little evidence; and instability, which would lead, not to a vast number of fragments, but to one large body and an attendant satellite of comparable size. The external causes again seem to be limited to two: material collision and tidal action. The former is unlikely, for it requires *two* planets to have revolved in the space between Mars and Jupiter; further, collision would probably have volatilised both. Tidal causes may again be divided into two classes: solar tides and planetary tides, of which the only important ones could have been raised by Jupiter. Now a solar tide could have caused disruption only if it had been magnified by resonance, and resonance, like instability, would have given rise to a planet with one satellite. The only alternatives left are therefore collision, which must be adopted if all else fails, and tidal disruption by a close approach to Jupiter. The hypothetical primitive planet, which will be denoted by M , would then have to be near aphelion, and be overtaken by Jupiter. The encounter would be of the class described by Jeans* as "slow," and if M and M_0 are the masses, r and r_0 the radii, and β a certain number equal to unity when M_0 is spherical, the distance between the centres of the masses must have been less than $2.20(M_0/M)^{\frac{1}{2}}\beta^{\frac{1}{2}}r$; and if ρ be the density of M and ρ_0 that of Jupiter, this reduces to something rather greater than $2.20r_0(\rho_0/\rho)^{\frac{1}{2}}$. Thus the planet M must have passed very close to the surface of Jupiter. This distance is practically unaltered if tensile strength is taken into account, so that a solid and a liquid planet would be almost equally easily broken up. The velocities of the fragments relative to their mean would be at least of the order of the velocity that would cause a particle on the surface of M to depart from its influence, and this is likely to have been about 2 km. per second, while the average departure of the orbital velocities of the asteroids from their mean is about 3 km. per second. These relative velocities might be in any direction, for M might pass Jupiter considerably to the north or south of the

* *Memoirs R.A.S.*, 62, pt. 1, 39-41.

orbital plane of the latter, and thus the orbits of the fragments might be considerably inclined, the possible inclinations being again of the same order as the actual ones. The aphelion distances of all would be almost equal to the distance of Jupiter when the encounter occurred. Being initially liquid or solid they would not dissipate, however small they were.

The subsequent history of these bodies would be determined by the resisting medium and by planetary perturbations. Inside the orbit of Jupiter the angular velocity of the medium would be greater than that of Jupiter, but probably all the asteroids and inner planets would be overtaking it, and would therefore approach the sun. Thus the large eccentricities and inclinations of the minor planets, and the facts that none move at a greater distance than Jupiter and most at much smaller distances, agree well with the theory. Some may have been captured and forced to become satellites of Mars and Jupiter; some may even have been precipitated on these planets. The major axis of the orbit of the primitive planet would be expected to be best preserved by the largest members; the correlation of eccentricity and inclination with size among the asteroids is utterly uncertain, on account of the great variation among their original values. The perturbations produced by the other planets would alter the positions of the orbital planes and of the lines of apses, and possibly set up large oscillations in the eccentricities.

IV. *The Formation of Satellites by Resonance.*

In a recent paper* the writer has examined Sir G. H. Darwin's "Resonance Theory" of the origin of the moon, and although a complete proof of it was not obtained, yet it was found that those aspects of it that could be tested corresponded strikingly with the facts. According to this theory the earth and the moon formerly formed one fluid body, rotating in about four hours; when this was true one of the free vibrations of the mass was of such a speed that the projections moved backwards round the equator relative to the body in a time equal to the solar day of that epoch. In other words, if one elevation started under the sun, it would always remain under it; hence such a displacement could be maintained without the application of any external force. Conversely there would be no internal force to oppose such a deformation. Now the attraction of the sun would apply forces tending to produce a semidiurnal tide of just this type, and in the absence of internal forces to resist the change the deformation would steadily increase. If it became large enough, which seems quite probable, the mass would break up in such a way as to form a satellite at a distance of the order of the original diameter and revolving in a period longer than the period of rotation. The subsystem would afterwards evolve under the influence of tidal friction till it bore a strong resemblance to our own.

* *Monthly Notices*, 78, 1917, 116-131.

The present section is an attempt to find out whether any other cases of resonance can have occurred in the solar system. First, suppose the whole mass and angular momentum of each subsystem to have been collected into one body, and let the angular velocity of that body be ω_1 . This can be directly calculated from observation. The angular velocity needed for resonance depends on the internal distribution of density. If it is ω_2 , and the moon is supposed to have been formed by resonance, then ω_2 for our subsystem is not greater than ω_1 . For physically similar bodies * ω_2^2 is proportional to the mean density. Now on the hypothesis that this holds for the planets, it will be possible to calculate ω_2 for each of them in terms of its value for the earth. In the following table this has been done; ω_2 has been put equal to ω_1 for the earth, which is an upper limit. The unit is $10^{-4}/1$ second.

	ω	ω_2	ω_1/ω_2
Mercury	?	3.9	?
Venus	0.74 ?	4.0	0.18 ?
Earth + Moon	4.1	4.1	1.0
Mars	0.71	3.5	0.20
Jupiter	1.74	2.05	0.86
Saturn	1.77	1.44	1.23
Uranus	1.60 ? †	2.0	0.80 ?
Neptune	?	1.95	?

The first striking fact about this table is that the available angular velocity for Saturn is greater than that needed for resonance, and hence, if it was once slightly distended, the condition for resonance must have been satisfied, and the question at once arises, why has Saturn not a satellite comparable in size with itself? The answer may be found either in a considerable departure from physical similarity or in the size of the disturbing force. On account of the greater distance from the sun, the absolute height of the equilibrium tide on Saturn is only $\frac{1}{20}$ of that on the earth, in spite of the lower density; hence the number of rotations needed to produce an amplitude comparable with the radius would be over one hundred and eighty times as great, and in this time resonance might have been destroyed by condensation. Further, if Saturn were physically similar to the earth and rotating with its actual speed, its ellipticity would be $\frac{1}{7}$ instead of its actual value $\frac{1}{9}$; this points to a stronger central condensation and the need of a greater velocity for resonance. Thus the absence of a very large satellite of Saturn is not surprising. The angular velocity needed

* If the radii of two bodies are a and a' , and points at distances r and r' from their centres are chosen such that $r'/a' = r/a$, they are called "physically similar" if the ratio of the densities at these points is independent of r .

† This speed of rotation is that obtained by Leon Campbell, H.C.O. Circular 200, from photometric measurements, and quoted in the *Observatory* for 1917.

for instability would be 2.04 in the same units, so that Saturn is stable.

For no other planet except the earth is the angular momentum at present in a subsystem enough for resonance; but the disturbance due to solar tidal friction must be considered. Its effect on the superior planets is probably small, but in the cases of Mercury and Venus it must be important. For if ω denote the angular velocity of rotation and n' that of orbital revolution, the part of $d\omega/dt$ due to solar tidal friction is proportional to $n'^4\rho^{-1}\sin 2\epsilon$, where ϵ is the lag of the tide. The variation of ϵ depends on the law of viscosity; in the case of small firmo-viscosity it is small and proportional to $\omega - n'$. Thus we can write

$$\frac{d\omega}{dt} = -A \frac{n'^4}{\rho} (\omega - n')$$

where A does not depend on the particular planet considered.

For the earth * $d\omega/dt$ is $620'' \div (1 \text{ century})^2$, of which about three-quarters or more is due to the moon and the remainder to the sun. We therefore find for Mercury

$$\frac{d\omega}{(\omega - n')dt} = \frac{1}{0.93 \times 10^8 \text{ years}},$$

and for Venus

$$\frac{d\omega}{(\omega - n')dt} = \frac{1}{3.8 \times 10^9 \text{ years}}.$$

Hence $\omega - n'$ for Mercury will be reduced to e^{-1} of its present value in 10^8 years, and for Venus a similar reduction must take about 4×10^9 years. This assumes, however, that internal viscosity in these planets has never been more powerful than in the earth at present, which is almost certainly untrue, for the earth must have formerly been much more plastic than it is now, and the same is probably true of all the other inner planets. Thus at some remote date, say 10^9 years ago, the values of $\omega - n'$ for Mercury and Venus must have been much greater than at present, and by a quite indeterminate amount. The primitive rotations of these planets may then have been very fast, and there is no reason as yet against their having been even faster than that of the earth. In fact, the effect of solar tidal friction on Venus must be twice or thrice, and on Mercury about one hundred times, that of the sun and moon together on the earth, if we adopt the results that attribute to them a rotation at present about as fast as that of the earth; and as the physical histories of these planets and the earth are probably not unlike, it follows that at previous epochs they must have been rotating faster than the earth. Thus the only way of avoiding the conclusion that the angular velocity of Mercury was greater than that needed for resonance when the moon was formed is to deny that Mercury is as old as the earth, which is a very difficult position to maintain. Indeed the angular momentum at

* *Monthly Notices*, 77, 453.

that time must have been enough not merely for resonance, but for actual instability, and therefore if Mercury rotates at present in any period near 24 hours it is certain to have produced a satellite of a diameter comparable with 1200 kilometres. If it always keeps the same face towards the sun this statement ceases to be necessarily true, but it cannot be directly contradicted. The same remarks apply to Venus with less cogency, for the frictional effect of the lunar tides was greater in the past; but if Venus is somewhat older than the moon it also should have produced a satellite. We are next led to inquire what can have become of such a satellite, if it existed. Its subsequent history would be very like that of the moon, the bodies separating on account of tidal friction and tending towards a state where each kept the same face turned towards the other; such a state would ultimately be attained unless some external cause altered the course of the evolution. In the terrestrial case the period of revolution when this state is reached will be about 44 days, but for the inner planets it is quite unknown. The destruction of a satellite would not form an attractive hypothesis, but the distance might have increased to such an extent that a stable orbit became impossible; the satellite would then depart widely from it and proceed to revolve independently about the sun. Mercury is the only body that can have been a satellite of Venus, and if it is assumed to have been, the difficulty about its own satellite disappears. Although this suggestion appears wildly improbable *a priori*, yet it has followed from the short period of rotation as a direct consequence of physical laws, and cannot at once be discarded. What can be done, however, is to adopt it tentatively and see whether it leads to an impossibility.

The problem is one of great mathematical difficulty, and it is not known how far away from its primary a satellite can have a stable variational orbit. The solution in trigonometrical series is known to be justifiable so long as n is greater than $3n'$, and probably the orbit is stable when n is still smaller. Taking the maximum mean distance to correspond to $n = 3n'$, we shall therefore underestimate it, and therefore overestimate the relative velocity of the two bodies. Now after the separation Mercury would move in such an orbit that the aphelion distance was nearly equal to the distance of Venus from the sun (supposing it to have been lost when between Venus and the sun), and its aphelion velocity would be practically the difference between the velocity of Venus relative to the sun and that of Mercury relative to Venus. These are respectively 35.2 and 0.7 kilometres per second; the latter corresponds to $n = 3n'$, and therefore is an upper limit. Now in the usual notation of the problem of two bodies the aphelion velocity is $\left(\frac{\mu}{a} \frac{1-e}{1+e}\right)^{\frac{1}{2}}$; and if v be the velocity in a circular orbit at the same distance, we have $v = \{\mu/a(1+e)\}^{\frac{1}{2}}$. Hence the difference is $v\{1 - (1-e)^{\frac{1}{2}}\}$, or practically $\frac{1}{2}ev$, and therefore the initial eccentricity was less than $2 \times 0.7/35.2$, or $\frac{1}{25}$. The subsequent action

of the resisting medium would reduce this, so that the present eccentricity of Mercury, if it were ever a satellite of Venus, must be less than $\frac{1}{2.5}$. It is actually $\frac{1}{5}$; we must therefore conclude that Mercury was never a satellite of Venus. Thus Venus can never have rotated fast enough to produce a satellite. Consequently it is impossible that the primitive rotation of Venus when condensation was complete was as rapid as that of the earth; and if we assume that it was never more than $4 \times 10^{-4}/1$ second, then it is not difficult to show that the time needed to reduce the rotation relative to the sun to zero, even on the hypothesis that the viscosity was such as to make the lag of the tides constantly equal to 45° , is of the order of 10^9 years.* Hence the present rotation of Venus may be either slow or fast—there is little to indicate which is correct. Mercury, on the other hand, would have its relative rotation reduced to a negligible amount in about 2×10^7 years, so that the time when tidal friction was producing its maximum effect was probably long enough for this to take place. The observational evidence on the rotation of these two planets is equivocal, some observers favouring a rotation in the period of revolution, and others one in about 24 hours. The evidence of cosmogony is strongly in favour of the slow rotation of Mercury, but can make no definite pronouncement with regard to Venus. At the same time no considerable effect can be produced by solar tidal friction on any of the superior planets except perhaps Mars.

V. Summary and Conclusions.

It is first shown that the existing planets cannot have been formed by the slow condensation of a gaseous mass moving in any plausible way, however its density was distributed. For this and other reasons the tidal theory of the origin of the solar system is adopted. In the second section the effect of the gaseous matter set free in the initial disruption and not collected into the nuclei is considered. On the supposition that it was of such density that, before its own viscosity caused it to dissipate, its resistance had reduced the eccentricities of planetary orbits to the extent required by the theory and by their present state, estimates of its density and of the age of the system are obtained. The latter agrees surprisingly well with the age of the earth determined by its radio-active constituents, though it rests on uncertain data and will need subsequent revision. After a short time the medium would be forced by the reaction of the great planets upon it to revolve around the sun so that the planets had little motion in

* In view of the fact that for the same lag the effect of the solar tides alone on Venus is greater than that of the solar and lunar tides together on the earth, while Darwin found that the changes in the moon's motion could have taken place in 5×10^7 years, the long period needed for Venus appears remarkable. The chief cause is that in the terrestrial case the lunar tides were formerly much greater than now, and that the time needed to produce a given effect on the rotation is thereby reduced to $\frac{1}{13}$ of what it would be were the moon's distance constant.

longitude relative to it. It is further shown that the angular velocity at any point of the medium multiplied by the fourth root of the distance from the centre is a function of the distance from the polar axis only. The great planets would be surrounded by distended masses of denser matter moving with them, and it is to the resistance of the medium to the large surfaces exposed by these that the comparatively small eccentricities of the great planets are to be attributed. The moon is the only satellite likely to have been less affected by the resisting medium than by tidal friction.

It is next shown that any planet or satellite whose present diameter is less than 1000 kilometres could never have been gaseous, as its gravitative power would have been unable to hold it together. The smaller members of the system must therefore have been liquid when first formed. The asteroids were probably formed from a primitive planet which approached Jupiter so closely as to be broken up by tidal action, and their present motions appear to agree fairly well with this theory.

The moon is the only satellite that can have been formed by resonance; but if Saturn was no more strongly condensed towards the centre than the earth is, it would have been likely to produce one in this way. The absence of such a satellite confirms the result indicated by the ellipticity that Saturn is on the whole relatively more dense in the centre than the earth. Adopting, on the other hand, the hypothesis that Mercury rotates in about 24 hours, it is shown that it must have once had a greater angular velocity than that needed for resonance. It is suggested that the absence of a satellite from Mercury might be accounted for if Mercury was itself once a satellite of Venus; but on examining this hypothesis quantitatively it is found that the eccentricity of the orbit could never have been so great as it actually is. Hence Mercury can never have rotated with the speed needed for resonance, and the action of solar tidal friction has been enough to make it always keep the same face towards the sun. The rotation of Venus is quite uncertain; and none of the superior planets except perhaps Mars can have been much affected by solar tidal friction. The only planets dominated by tidal friction are therefore Mercury and the earth.

The Chromospheric and Coronal Spectrum ($\lambda 6000 - \lambda 7600$) in the Total Solar Eclipse, 1911 April 28. By the Reverend A. L. Cortie, S.J.

The present series of measures of the wave-lengths of some chromospheric lines in the region $\lambda 6000 - \lambda 7640$ is derived from measurements made on a plate which was taken, under adverse circumstances, through cirrus clouds, during the total solar eclipse of 1911 April 28, observed at Vavau, Tonga Islands, South Pacific. The plate is very dense and is difficult to measure. But it is in

good focus from beyond the H and K lines to $\lambda 7640$ in the red. As, however, copious wave-length tables of the chromospheric spectrum in the more refrangible part of the spectrum have already been published by Lockyer, Evershed, Dyson, Mitchell, Adams, and others, and the dispersion of the plate is very small, covering only 34.5 mm. from the K line to $\lambda 7640$, it would have been useless to measure the comparatively few lines in these parts of the spectrum.

The spectrum plates of the flash obtained in 1896 and 1898, by Sir Norman Lockyer and by Mr. Evershed, show the H_{α} line of hydrogen. The plates obtained by Dr. S. A. Mitchell in 1905 have been measured to $\lambda 6191.6$ in the red. Mr. Walter S. Adams and Miss Cora G. Burwell have published tables of "The Flash Spectrum without an Eclipse" extending from $\lambda 4800$ to $\lambda 6600$. It seemed advisable therefore in the present instance only to publish measures from $\lambda 6000$. The wave-lengths between $\lambda 6000$ and $\lambda 6600$ are in quite satisfactory agreement with those given by Adams and Burwell,* which served as a test of the accuracy of the measures. Beyond $\lambda 6600$ there are measurements of twenty-five new lines. The linear dispersion of the plate between $\lambda 6000$ and $\lambda 7638$ is 7.06 mm. The wave-lengths therefore have only been given to 1 Å. The accuracy is probably of this order in the shorter wave-lengths measured, and in the extreme wave-lengths about 3 Å, which is the value corresponding to 0.01 mm. in the screw of the measuring instrument.

The prismatic camera with which the plate was taken was under the charge of Father E. Pigot, S.J., Director of the Observatory of St. Ignatius College, Riverview, Sydney, N.S.W. It consisted of a prism of dense flint glass 7 inches edge and refracting angle 40° , made for the Joint Permanent Eclipse Committee by Sir Howard Grubb for the eclipse of 1905. The deviation of the prism for H_{β} is 28.5° . At the eclipse the table upon which the prism and its camera were rigidly fixed was tilted through an angle of 55° , in order to obtain concentric and symmetrical arcs in the flash spectrum. The attached camera had a portrait lens by Dallmeyer of 6 inches aperture and 30 inches focal length. The plate was one of those exposed to obtain the flash spectrum and the coronal rings during totality. All the others were spoiled by the clouds. On this plate the flash spectrum was impressed as the shutter was being closed at the end of totality.

In the table the first column gives the wave-lengths determined. These were found by means of the graphical construction for the application of the Hartmann-Cornu formula devised by Dr. Henroteau,† supplemented by ordinary wave-length curves. The intensities of the lines are given on a scale of 0, a line just discernible, to 10, where H_{α} , by far the brightest arc on the plate, is reckoned 40. The probable identifications of the lines are given by Rowland's Tables of Wave-Lengths, by the chromospheric lines determined by Adams and Burwell, and for the iron lines, which

* *Astrophysical Journal*, 41, 116 (1915).

† *Monthly Notices*, 77, 77.

constitute the majority of those measured, by the "Arc Spectrum of Iron," published by Dr. Keivin Burns.*

The photographic plate on which the spectrum was impressed was a panchromatic plate of Wratten & Wainwright. The difficult measurements on the indifferent photograph obtained under the adverse circumstances, and the reductions and identifications of the lines, have taken a very considerable amount of time. Although H_{α} is very bright, the red helium lines are only moderately bright, and D_3 just easily discernible.

Spectrum of Chromosphere.		Probable Identification.				Remarks.
Wave-length.	Intensity.	Element.	Sun. Rowland.	Chromosphere. Adams.	Iron-arc. Burns.†	
6003	3	Fe	03'2	03'4	03'0	Mitchell 02'9.
6024	3	Fe	24'3	24'0	24'1	Mitchell 24'1.
6148	3	Fe	48'0	47'9	47'9	
6151	4	Fe	51'8	51'8	51'6	51'1 Y?
6200	4	Fe	00'5	00'6	00'3	Mitchell ends 6191'6.
6220	2	Fe	19'5	19'6	19'3	
6248	0	Fe	47'8	47'8	...	Enhanced line.
6281	5	Fe	80'8	80'7	80'6	
6306	5	Sc	05'9	05'9	06'1	Fe?
6337	8	Fe	37'0	37'3	36'8	
6347	2	Si	47'3	47'3	...	Fowler Si 47'3.
6368	2	Fe	69'7	69'7	...	Identification doubtful.
6390	0	La	90'7	90'7	...	
6432	4	...	32'9	32'9	...	Rowland Fe?
6448	1	Ca	50'0	49'8	...	Identification doubtful.
6489	5	Ti	91'8	91'8	...	Enhanced line.
6500	2	Ca	99'9	99'4	...	6499'2 Fe.
6516	8	Fe	16'3	16'3	16'1	Enhanced line.
6563	40	H	...	...	...	Brightest arc on plate.
6585	1	Ni	86'6	86'5	...	Adams ends 6599'3.
6608	9	Fe	09'4	...	09'3	
6640	5	...	39'9	...	...	
6678	2	He	...	...	...	6678'1 Merrill. Not very bright.
6715	3	Fe	15'6	...	15'4	
6731	...	Fe	33'4	...	33'1	Identification doubtful.
6752	3	Fe	53'0	...	52'7	
6792	2	Fe	93'5	...	93'3	
6806	4	Fe	07'1	...	06'9	
6863	1	Fe	62'2	...	61'9	

* *Lick Observatory Bulletin*, No. 247.

† International scale.

Spectrum of Chromosphere.		Probable Identification.				Remarks.
Wave-length.	Intensity.	Element.	Sun. Rowland.	Chromosphere. Adams.	Iron-arc. Burns.*	
6910	1	Fe	...	...	11.5	
6941	.10	...	41.5	...	...	
6976	4	Fe	76.5	...	76.3	
7027	0	Fe	27.7	...	27.6	
7066	3	He	...	...	...	7065.2 Merrill.
7113	3	Fe	12.5	...	12.2	
7124	0	...	22.5	...	...	
7176	1	Fe	76.2	...	75.9	
7227	3	Ba?	27.8	...	28.8	
7239	4	Fe	40.1	...	39.9	Rowland ends 7331.
7282	3	He	...	...	...	7281.3 Merrill.
7402	2	Fe	...	...	01.7	7401.9 Higgs' faint double.
7413	1	Fe	...	...	11.2	7411.0 Higgs' double.
7476	1	...	...	...	...	7476.3 Higgs' faint double.
7551	1	...	...	...	...	7551.2 Higgs' faint line.
7638	0	...	...	...	...	7638.5 Higgs' solar line in A band.

The coronal spectrum consists of a dense continuous band. Near the helium line 7066, but separable from it, is a fairly bright arc, extending over 120° , which is possibly a new coronal radiation. It differs in curvature from the arcs of the hydrogen lines, and possibly was impressed upon the plate while it was being exposed to the coronal spectrum during totality. The flash spectrum lines obtained at the end of totality were adventitious. The difference in curvature from the hydrogen and helium fiducial arcs makes it difficult to determine its exact wave-length. The only explanation that suggests itself for this asymmetry in curvature, unless the coronal arc is a photographic freak, is that the plate-holder was slightly shaken when being closed at the end of totality, at which moment the chromospheric arcs were impressed upon it. But, by placing the measuring wire tangent to H_α and the ring, at corresponding curvatures, the resulting wave-length is $\lambda 7107$. The greatest distance between the H_α arc and the coronal arc would give a resulting wave-length $\lambda 7150$. This value is tempting, as the $\lambda 5535.8$ of a well-known coronal line, multiplied by Nicholson's factor 1.292,† would give a line at $\lambda 7152.3$.

No other coronal rings can be distinguished on the background of a continuous spectrum.

Stonyhurst College Observatory ;
1918 April 6.

* International scale.

† *Monthly Notices*, 76, 415.

Anastigmatic Eyepieces. By A. E. Conrady.

All eyepieces at present known have either a curved field (*i.e.* require a different focal adjustment, or a change of accommodation of the eye, for successive zones) or else they display astigmatism and are then incapable of giving sharp definition in the outer parts of the field of view.

This unsatisfactory state of affairs results from non-fulfilment of the so-called Petzval condition, which was known in a limited form to Airy, was stated without proof by Petzval, and was proved, as far as first order aberrations are concerned, by Seidel, being the fourth of his famous five conditions which a lens-system ought to fulfil.

The condition may be most clearly stated by the equation

$$1/R_i = -1/N_1 f_1 - 1/N_2 f_2 - , \text{ etc.},$$

in which R_i signifies the radius of curvature of the surface on which the sharp image lies, N the refractive index of the several simple lenses of which the lens-system is composed, and

$$f = (N - 1)(1/r_1 - 1/r_2)$$

the focal length of an infinitely thin lens having the same curvatures as the real lens, the thickness of the latter not entering into the expression. The negative signs on the right merely express the fact that with collective lenses (f positive) the image is curved away from the observer. The equation shows that a single lens can have a flat image free from astigmatism—which is now generally known as an anastigmatic image—if it is a meniscus with the same radius of curvature on both sides; such a lens is possible, as the finite thickness gives it a collective effect, and is in fact realised in the Goerz Hypergon, a photographic objective with a flat field of nearly 140° angular extent. Unfortunately the power of such a meniscus is too feeble to make the application of this simple principle possible in the construction of eyepieces.

For ordinary lenses such as are used in eyepieces, the Petzval equation proves a radius of curvature of the sharp image of N times, roughly one and a half times, the focal length of a single lens; but when two lenses are widely separated to secure achromatism of the field, as in the Huyghenian and approximately in the Ramsden eyepiece, then the combined power of the two lenses is twice that of the resulting eyepiece, and the sharp field of these eyepieces therefore has a radius of curvature equal to about three-quarters of their equivalent focal length. The use of an achromatised eye lens (as in the "Kellner" and "orthoscopic" types) does not improve matters sensibly; any flattening of the field of such eyepieces is always obtained at the expense of astigmatism. But *closely spaced* achromatic combinations, which, however, are not much in use, are capable of better results and can be made to

increase R_4 to fully one and a half times the equivalent focal length if dense Barium glasses are used.

It was stated in the opening paragraph that the accommodation of the eye can counteract the curvature within limits. Very young observers can thus obtain sharpness all over the field with all usual eyepiece powers, but the range of accommodation becomes rapidly less in middle age; if we allow two diopters, or a range from half a metre to infinity, as a reasonable average, then it is easily shown that for Huyghenian and Ramsden eyepieces the usual apparent field of about 40° can be seen sharp at one setting of the focus only if the equivalent focal length of the eyepiece is not less than about one inch. With well-designed eyepieces of the closely spaced achromatic type the limit goes down to about half an inch equivalent focal length. It is, therefore, chiefly for eyepieces of high power that anastigmatic correction is of importance, and the method of achieving this, which forms the proper subject of this note, is, fortunately, particularly applicable to these and brings with it other advantages.

Owing to the considerable focal length of all astronomical objectives, the contribution of the latter to the total curvature of the image is negligible. Applying the Petzval equation to the eyepiece only, and neglecting variations of refractive index, it tells us that the powers of its lenses must add up to zero. That is only feasible if we can build a satisfactory eyepiece from a combination of *concave* and *convex* lenses of *approximately the same focal length*. This can be done. If we place a concave lens at a proper distance within the primary focus of the objective, it will lengthen the cones of light which it intercepts and will bring them to focus in an enlarged real image; it will, in fact, act like the old familiar Barlow lens. This enlarged image can then be further magnified by any of the well-known types of eyepieces, and it is a simple problem of exact optical computation to determine the data so that an anastigmatically flat field is secured.

The novelty of the proposal lies in the fact that, whilst the old Barlow lens was a nondescript achromatic concave of comparatively long focal length, used in combination with the ordinary eyepieces of short focal length belonging to the telescope, which at best could produce only a slight improvement of the curvature of the image, I propose a carefully computed approximately achromatic negative lens, which will magnify the primary image about four times, and will usually be of *shorter* focal length than the eyepiece to be used with it, and the latter is also to be specially designed to produce the desired combined effect of a perfectly flat image free from astigmatism.

It is quite easy to design such a system, which consists of two separated components only—a cemented concave and a cemented convex one—and which, therefore, has the same light-transmitting power as the ordinary eyepieces; but the use of a binary type for the convex part will allow of reaching a much larger angle of field.

Incidental advantages of the system are that by adjustment of the separation between the concave and convex elements, the magnifying power can be varied within a reasonable percentage of that for which the set has been computed without sensible loss of quality, and that the best possible observing conditions can thus be established, according to the brightness of the object or the state of the atmosphere; also that the eyepoint will lie about four times as far from the last lens as it does in an ordinary eyepiece of the same magnifying power.

The principle can also be applied to terrestrial telescopes, especially those of the prismatic type; but in this case the objective should be included in the computation, as its contribution to curvature and astigmatism cannot be neglected.

This note is published in the hope that designers will work out formulæ for their respective firms, and that eyepieces of this highly desirable type will thus become commercially obtainable at an early date.

*Technical Optics Department,
Imperial College of Science and Technology,
South Kensington, 1918 April 10.*

Orbit of Planet DB (Alinda). By A. C. D. Crommelin.

An orbit has been derived from the following three photographic positions, of which the first two were obtained by Prof. Wolf at Königstuhl, and the third by Mr. Davidson with the 30-inch reflector at Greenwich. The positions are for 1918.0 and are corrected for parallax, the assumed values of $\log \Delta$ being 9.3351, 9.4753, 9.7129 respectively.

	G.M.T. 1918		R.A.		N. Dec.
	h m		h m s		° ' "
Jan. 3	8 21.96		5 16 27.04		12 26 16.37.
Feb. 5	8 37.44		6 56 6.03		34 19 28.27.
Mar. 9	9 6.74		8 33 29.51		35 50 41.35.

The following are the elements that have been deduced:—

$$\begin{aligned}
 T &= 1918 \text{ Jan. } 3^{\text{h}} 6^{\text{m}} 8.13^{\text{s}} \text{ G.M.T.} \\
 \omega &= 347^{\circ} 45' 50''.87 \\
 \Omega &= 110 \quad 54 \quad 5.83 \\
 i &= 8 \quad 57 \quad 21.24 \\
 \phi &= 32 \quad 10 \quad 4.64. \\
 \mu &= 883''.081. \\
 \log a &= 0.402671. \\
 \log q &= 0.072542. \\
 \text{Period} &= 4^{\text{y}} 01795.
 \end{aligned}
 \left. \begin{array}{l} \\ \\ \\ \\ \end{array} \right\} 1918.0.$$

Constants of equator—

$$\begin{aligned} x &= [9.9953581] r \sin (v + 188^\circ 54' 3''.84). \\ y &= [9.9729649] r \sin (v + 101^\circ 58' 9''.01). \\ z &= [9.5702738] r \sin (v + 77^\circ 22' 2''.76). \end{aligned}$$

The elements differ very slightly from those of Stracke, but are entitled to greater weight, owing to the longer interval covered by the observations, and the nearer approach to equality in the time intervals.

The near approach of the period to four years implies that a favourable opposition occurred in 1914, and another will occur in 1922. Outline ephemerides for these oppositions are appended; the first in the hope that images of the planet may possibly be found on plates taken four years ago.

Greenwich Noon.	R.A.	N. Dec.	log Δ .
	h m s	° '	
1913 Dec. 18	7 0 34	4 45	9.3473
28	7 26 22	10 58	9.3180
1914 Jan. 7	7 52 39	18 24	9.3155
17	8 16 20	25 23	9.3452
27	8 38 8	30 37	9.3995
1921 Dec. 31	3 29 52	4 39	9.4541
1922 Jan. 10	3 50 43	12 6	9.4572
20	4 19 20	19 43	9.4766
30	4 55 14	26 21	9.5134
Feb. 9	5 37 2	31 23	9.5627

The planet is so small that it will be nearly hopeless to detect it except at perihelion oppositions.

The fact that the period approximates to one-third of that of Jupiter will make the perturbations by that planet specially interesting. In the coming revolution there will not be a near approach to Jupiter.

Observations of Encke's Comet (1917 c.) made with the 28-inch Equatorial at the Royal Observatory, Greenwich.

(Communicated by the Astronomer Royal.)

G.M.T.	R.A.			Decl.			Star.
	Comet - *.	Corr. for Refr.	Log Factor of Parallax.	Comet - *.	Corr. for Refr.	Log Factor of Parallax.	
1918. d h m s	s	s					
Mar. 9 7 17 59.74	- 82.38	+ .08	9.5699	...	...	...	a
7 22 38.15	- 116.99	+ .03	9.5709	...	...	...	b
7 36 14.59	...	...	...	+ 178.73	- 2.03	0.8241	b
Mar. 12 7 22 25.54	+ 138.32	+ .14	9.5717	...	...	...	c
7 33 23.82	+ 70.85	+ .20	9.5727	...	...	...	d
7 55 19.51	...	...	...	+ 656.19	+ 18.18	0.8335	c

Places of Comparison Stars.

Star.	Name.	Mag.	R.A. 1918°0.	Decl. 1918°0.	Reduction to	Authority.
					Apparent Place.	
			h m s	N. ° ' "	s " "	
<i>a</i>	B.D. +11°122	8·3	0 53 35·34	N. 11 35 50"09	+0°54 +4'42	Leipzig A.G. Cat.
<i>b</i>	B.D. +11°124	8·5	0 54 10·45	11 41 3'62	+0°54 +4'43	"
<i>c</i>	B.D. +11°130	8·2	0 55 55·20	11 29 33'61	+0°54 +4'25	"
<i>d</i>	B.D. +11°135	8·0	0 57 3'97	11 28 14'28	+0°54 +4'26	"

There were 3 comparisons with star *a* in R.A., 2 with star *b*, 5 with star *c*, 3 with star *d*; 3 with star *b* in Declination, 2 with star *c*.

At the time of the comparisons with star *c* in Declination on March 12, the Z.D. was 84°. The observer noted these observations as uncertain. Possibly the comet should be further north by one revolution of the micrometer, or 12"·06. It is only on this night that there is any doubt about the revolution.

Assuming the values of $\log \Delta$ to be 0·0282, 9·9985 on the two days, the following apparent places of the comet have been deduced (corrected for parallax):—

G.M.T.				Apparent R.A.	Apparent N. Decl	Star.
d	h	m	s	h m s	° ' "	
Mar. 9	7	22	38·15	0 52 14'31		<i>a</i>
	7	22	38·15	0 52 14'38	11 44 11'10	<i>b</i>
12	7	33	23·82	0 58 15'35	11 41 4'55	<i>c</i>
	7	33	23·82	0 58 15'94	...	<i>d</i>

The following notes were made on the appearance of the comet by the observer, Mr R. Jonckheere:—

March 9: More central condensation than on March 1; magnitude 8·3 (B.D. scale).

March 12: Comet much sharper, brighter, and smaller; its diameter was $1\frac{1}{2}'$, magnitude 7·7 (B.D. scale). Its aspect in the 6-inch Corbett telescope was almost stellar, but in the 28-inch no definite nucleus could be seen.

Royal Observatory, Greenwich:
1918 April 18.

